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A History of psychology in
autobiography

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY

VOLUME I

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By

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Carl Emil Seashore
C. Spearman
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Theodor Ziehen
H. Zwaardemaker

Edited by
Carl Murchison

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THE
INTERNATIONAL UNIVERSITY SERIES
IN
PSYCHOLOGY

Edited by
CARL MURCHISON, Ph.D.
Professor of Psychology and Director of the
Psychological Laboratories in Clark University

THE INTERNATIONAL UNIVERSITY SERIES IN PSYCHOLOGY

PSYCHOLOGIES of 1925

By Madison Bentley, Knight Dunlap, Walter S. Hunter, Kurt Koffka, Wolfgang Köhler, William McDougall, Morton Prince, John B. Watson, and Robert S. Woodworth. *Edited by* Carl Murchison.

CRIMINAL INTELLIGENCE

By Carl Murchison, Ph.D., *Professor of Psychology and Director of the Psychological Laboratories in Clark University.*

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By James Mark Baldwin, Mary Whiton Calkins, Edouard Claparède, Raymond Dodge, Pierre Janet, Joseph Jastrow, F. Kiesow, William McDougall, Carl Emil Seashore, C. Spearman, William Stern, Carl Stumpf, Howard C. Warren, Theodor Ziehen, and H. Zwaardemaker. *Edited by* Carl Murchison.

PREFACE

The author of a recent history of psychology found that it was impossible to get important facts concerning the scientific development of certain individuals except from those individuals themselves. Since a science separated from its history lacks direction and promises a future of uncertain importance, it is a matter of consequence to those who wish to understand psychology for those individuals who have greatly influenced contemporary psychology to put into print as much of their personal histories as bears on their professional careers.

The initial idea, which later developed into the general plan for *A History of Psychology in Autobiography,* was contained in a letter of April 10, 1928, from Edwin G. Boring to Carl Murchison. Shortly afterwards, there was a conference in Emerson Hall, participated in by Edwin G. Boring of Harvard University, Karl Bühler of the University of Vienna, and Carl Murchison of Clark University, which resulted in our inviting Herbert S. Langfeld of Princeton University and John B. Watson of New York City to join with us in a committee which would assume responsibility for all invitations extended for inclusion in such a series.

We then proceeded in the following manner in arriving at a tentative list of individuals to whom invitations were to be sent. Without consultation, each member of the Committee compiled a list of one hundred names that he considered eligible for such a series. The five lists of one hundred each were then combined, making a composite list of one hundred eighty-four names. Taking the one hundred eighty-four names as a nomination list, each member of the Committee then voted for sixty names. All chosen unanimously were placed upon a preferred list to whom invitations would be sent as the Series progressed.

Since preparing the original list, the members of the Committee, in conference, have expanded somewhat the principle of selection which originally guided them. We agreed that individuals on the fringe of psychology or even in a neighboring field of science might so influence psychology that they should be included.

It must not be thought that individuals were invited in order of eminence or of seniority exclusively. There are many factors which determine the order in which autobiographies appear in this Series and it is very probable that some of the most eminent individuals will appear in later volumes rather than in earlier ones. In two or three cases, we decided that certain individuals of the greatest eminence should probably not appear in the first one or two volumes because they are already well known to most psychologists and have recently been discussed at length in other histories, especially in Boring's *A History of Experimental Psychology*. During the next year, perhaps three volumes altogether will have appeared, while a fourth volume will appear in the near future. If our idea proves to be as fruitful as we now believe that it will, it is to be expected that additional volumes will appear at intervals of three or four years.

In this Series, the printing of long and comprehensive bibliographies has been definitely avoided, since these will be accessible in the *Psychological Register*.

CLARK UNIVERSITY
Worcester, Massachusetts
May 22, 1930

CARL MURCHISON, *Chairman*
EDWIN G. BORING
KARL BÜHLER
HERBERT S. LANGFELD
JOHN B. WATSON

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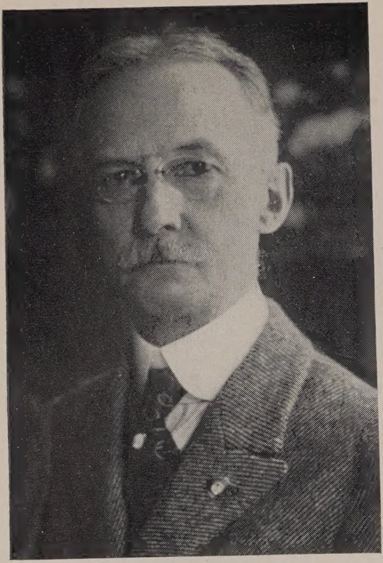

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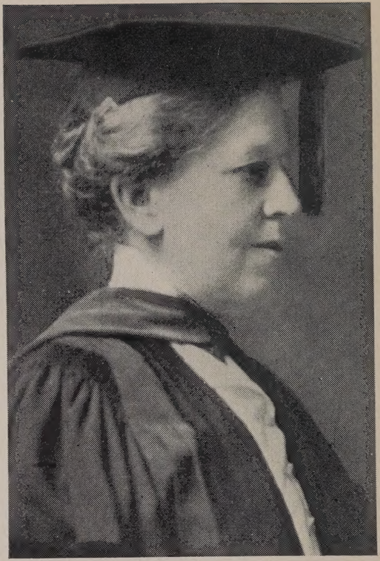

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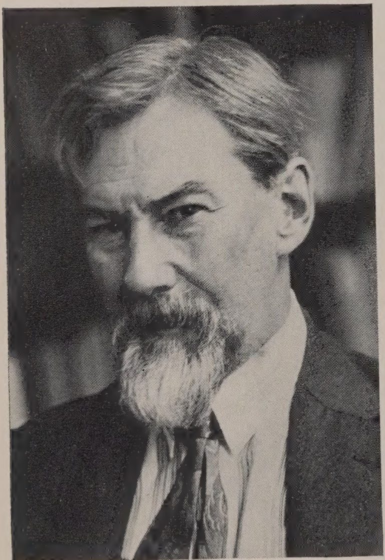

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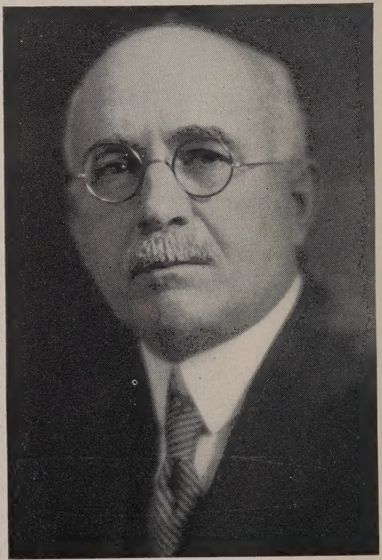

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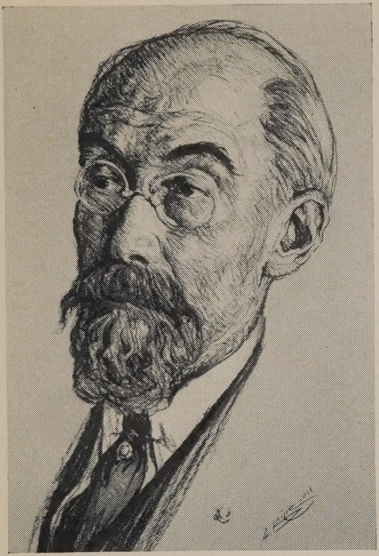

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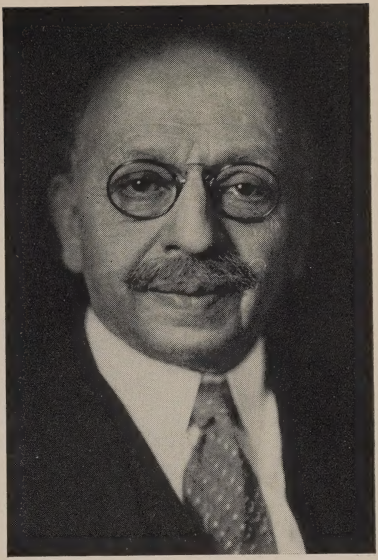

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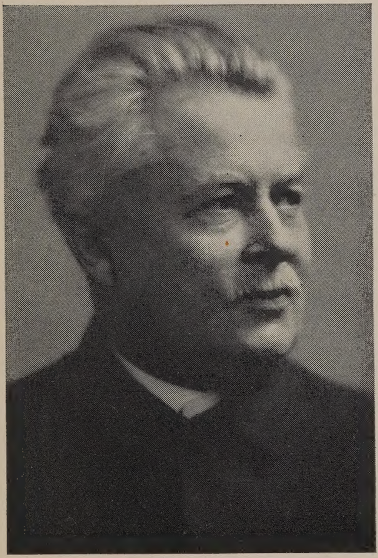

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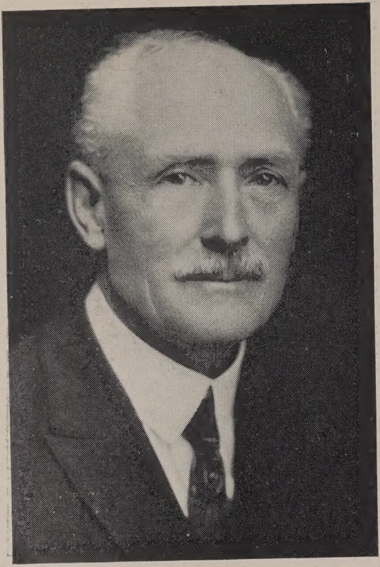

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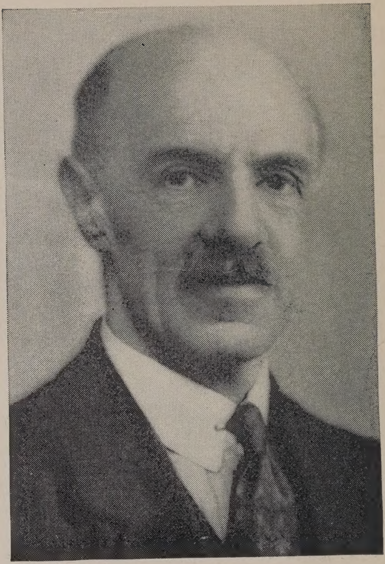

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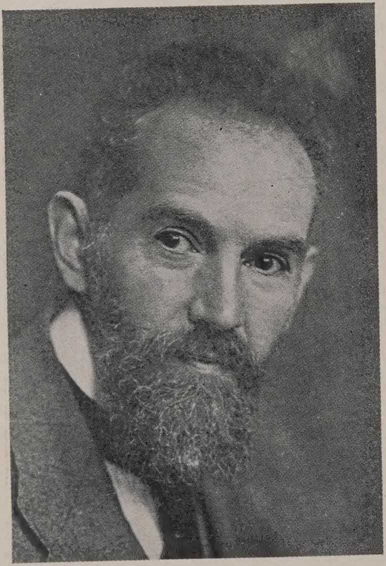

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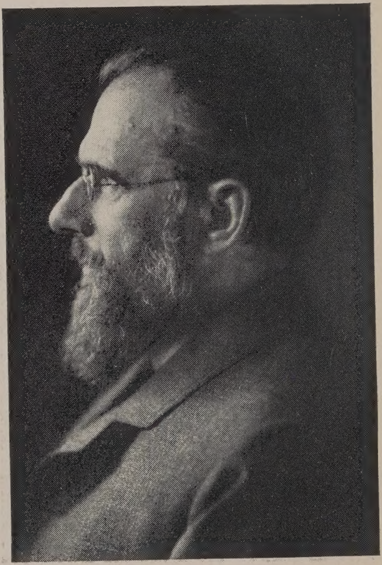

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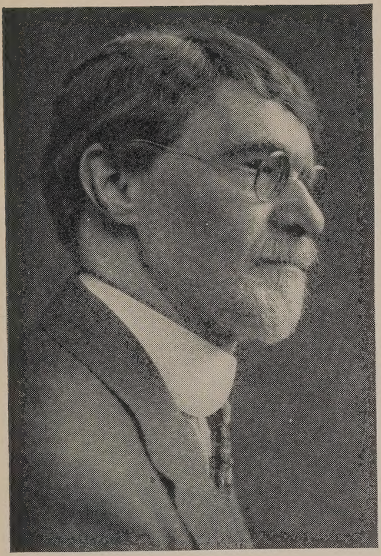

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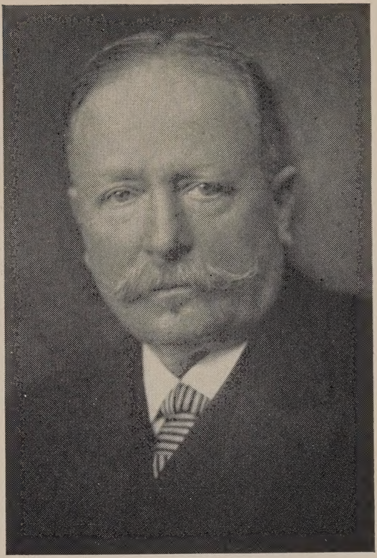

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H. Zwaardemaker

JAMES MARK BALDWIN

Introduction

It is a natural thing for a man who is approaching the end of his active career to cast an eye backward over the course he has run and assess the whole. It is interesting to any man to attempt to trace out the spiritual factors—the changing motives, interests, problems—of his life-work. To the psychologist this would appear to be a particularly spontaneous and natural thing to do, seeing that his own soul, like those of his subjects in the laboratory and in the world, is the sort of object he has spent his life teasing, analyzing, and estimating. So, when he is asked to set down the results, he finds his pages already prepared in reminiscence.

The account asked of me is the more easily checked up, seeing that such an account is in my case strewed along the course; the way has been littered with publications. The series of the writer's books and papers, beginning with a translation from the French in 1885, and probably not yet entirely finished, gives the line of the spiritual trajectory by a method more accurate than any other he might now devise.[1]

The writer's interest was directed towards psychology both through his early intention to go into the ministry and, more especially, by the undergraduate courses given in Princeton College in the eighties. President McCosh taught his Natural Realism vigorously, and the nucleus of all his instruction was empirical psychology.

---

[1] The titles themselves give the keynotes, and the dates (in each case the first edition) show the order: General Psychology: *Handbook of Psychology* (Vol. I, 1889; Vol. II, 1891) and *Elements of Psychology* (1893). Experimental Psychology: translation of Ribot's *German Psychology of Today,* papers collected in *Fragments in Philosophy and Science* (1902), in *Princeton Contributions to Psychology* (1894 ff.) and in the *Psychological Review* (1893 ff.). See also the popular book, *Story of the Mind* (1898). Child Psychology and Racial Psychology: *Mental Development in the Child and the Race* (1894, 3rd ed., 1906). Social Psychology: *Social and Ethical Interpretations* (4th ed., 1906) and *The Individual and Society* (1910). Genetic Psychology and Evolution: *Development and Evolution* (1902). *Darwin and the Humanities* (1909). Genetic Logic: *Thought and Things or Genetic Logic* (3 vols., 1906-1911). Philosophical Interpretation: *Genetic Theory of Reality* (1915) and *History of Psychology* (2 vols., 1913). Terminology: *Dictionary of Psychology and Philosophy* (4 vols., 1901-1906). Practical Studies (war period): *American Neutrality* (1916); collected papers in *Between Two Wars* (Vol. II, 1926); *The Super-State* (1916); *France and the War* (1916).

This was at that date—in contrast with the "rational" psychology—a sort of propadeutic to metaphysics and theology, taught in most of the American Colleges. McCosh, further, in two other points, had insight which was for that time prophetic, points which were to become leading motives later on in my own work: he had seized upon the project of scientific psychology as announced in Wundt's *Physiologische Psychologie,* then just out, and had also pronounced in favor of the theory of biological evolution, holding it to be consistent with the "divine government of the world," as explained in his work of that title. Furthermore, I was brought into the circle of interest in physiological psychology through the tradition of a course of readings in Wundt, arranged by McCosh, with the demonstrations given by W. B. Scott and H. F. Osborn, young members of the Princeton faculty. Under these influences, on graduating in 1884 and taking the "Mental Science Fellowship" offered to the class, I went to Germany for two semesters' study. Coming back to Princeton as instructor in 1886, I pursued courses in apologetics and theology in the Princeton Theological Seminary, all the while growing more and more disposed to accept a position in philosophy and psychology such as that which was soon (1887) offered me at Lake Forest University, Illinois.[2]

I. GENERAL, CHILD, AND EXPERIMENTAL PSYCHOLOGY

In Leipzig, Wundt was the rage. His laboratory and lectures were crowded, and it seemed that he, along with Fechner and Lotze, whose works were hardly mentioned by him, however, was laying the foundation of a really scientific and experimental "discipline." I was caught in the movement; and, while a brief stay in Berlin opened to me the high thought of Spinoza (studied in the seminar of Paulsen), the principle result of the German visit was a sort of apostolic call to the "new psychology." Finding in the literature the book of Ribot, *Psychologie allemande contemporaine,* which gave a resumé of the movement, I got the rights and made the English translation which appeared in 1880 as *German Psychology of Today.*

The interest in experimental psychology was not subordinate to that in philosophy and theology; but, on the contrary, it increased in force as I took up academic teaching. The exigencies of classroom work required the exposition of general psychology, for which proper textbooks were not in circulation. Sully's *Outlines* was the only available text in English (with Bain's two volumes for collateral reading). This pedagogical need motived the first volume of my *Handbook of Psychology, Senses and Intellect,* but the essential novelty and attractiveness of the problems of the second volume, *Feeling and Will,* carried me into less conventional and more personal research.[3] Certain positions taken up in the second volume, published while I was at Toronto (1900-1903), set the direction of later work. The principles of dynamogenesis, kinaesthesis, and those of active or motor interpretation of many of the mental functions were worked out. In this, I was led to abandon the older association and structural psychology in favor of functional and developmental views. The theory of synthesis (in such problems as those of apperception, conception, and volition) was based on motor synergy and adaptation. The motor theory of attention was developed a little later on.[4]

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[2] Subsequent academic terms of service (Toronto, Princeton, Johns Hopkins, Mexico, and Paris), together with other biographical details, may be found in the author's book of memoirs entitled *Between Two Wars, 1861-1921* (1926, Vol. I).

These tendencies were reinforced also by the sensational discovery about that time of the principal facts of hypnotism and suggestion. I went to France and got full information from both the Paris (Charcot, Janet) and Nancy (Bernheim) schools. This experience, supplemented a little later on by acquaintance with the work of the French school in the realm of the subconscious, was utilized in my books on *Mental Development.*

At this time I was given the means to found the laboratory of psychology in the University of Toronto—the first anywhere on British soil. A similar opportunity presented itself at Princeton in 1893. There, also, a laboratory was founded and regular courses instituted in experimental psychology. Of my own researches conducted at Princeton, the most important, I think, were on the "type theory of reaction" (establishing that the reaction-time varies with the type, as sensory, motor, etc., of the subjects; each type reacting most effectively through its preferred sense; a theory established at about the same time also by the experimental work of Flournoy of Geneva); and the optical illusion known by my name, viz., the displacement of the observed mid-point between two areas of different sizes—the mid-point being displaced toward the larger of the two areas. This latter has important bearings in the appreciation of architecture, the arrangement of the units of flat surfaces, decoration, etc.

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[3] The two volumes were abridged in the smaller textbook, *Elements of Psychology* (1893).

[4] I remember a remark made to me by Münsterberg in the summer of 1900: "You and I," said he, "are the 'motor men' on the psychological car."

The experimental vein was worked, though with lessening interest, for the ten years of my stay at Princeton. It flared up a bit when, on going to The Johns Hopkins in 1903, I was called on to set up another laboratory, my third; but the later development of this laboratory was due to a colleague, Stratton. Already at Princeton the new interest in genetic psychology and general biology had become absorbing, and the meagerness of the results of the psychological laboratories (apart from direct work on sensation and movement) was becoming evident everywhere. I began to feel that there was truth in what James was already proclaiming as to the barrenness of the tables and curves coming from many laboratories.

II. GENETIC AND SOCIAL PSYCHOLOGY: CIRCULAR REACTION, THE SOCIUS, SOCIAL HEREDITY

It was with the birth of the first child, Helen (the "H" of the books on mental development), that interest in the problems of genesis—origin, development, evolution—became prominent; the interest which was to show itself in all the subsequent years. "H" became (with, later on, her sister "E"), from her extreme infancy, the focus through which all the problems of general biology and psychology presented themselves. The series of experiments recorded in the book *Mental Development in the Child and the Race* opened the way to the study of the correlation of data with those of biology: experiments on right- and left-handedness, on color-perception, on suggestion, on imitation, on speech, etc. Such correlations were found in the theories of recapitulation, accommodation, and growth in biology. In the field of folk psychology, theories of conscious imitation, learning, social response and organization were worked out—these latter recorded in the second volume, entitled *Social and Ethical Interpretations.*

The outcome on the psycho-biological side was embodied in the principle of circular reaction, found to be the fundamental method of fruitful organic reaction to the environment of things and persons. On variations of this original act of life arise the main adaptations: conscious and social accommodation, imitation, invention, and volition (through the experience of "try, try again");[5] and on it organic evolution and social progress alike rest. This is the broad conclusion reached; it involved a radical modification of the current Spencer-Bain theory of the action of pleasure and pain in the economy of organic and psychological adjustment.

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[5] A procedure raised in physical science to the dignity of the "method of trial and error." We have here its spontaneous form.

On the psycho-sociological side the same principle operates without break or discontinuity, revealing itself in the various phases of suggestion and imitation. Through conscious imitation and its variations and oppositions, vistas open up along the great highways of individual and social progress. It is through intercourse with others, thus established, that the individual self-thought or *"ego"* is attained, along with its correlative term, the social fellow or *"alter,"* each using a common body of experiences and forming an identical social fellow or *"socius."* In each social situation the *socii* are in large measure identical, only partially and progressively different.[6]

This give-and-take, essentially imitative, constitutes a "dialectic of personal growth," which is at the same time that of social organization. Society, genetically considered, is not a composition of separate individuals; on the contrary, the individuals are differentiations of a common social protoplasm.[7] The conclusion is drawn that the individual is a "social outcome not a social unit." We are members one of another. The oppositions, conflicts, antinomies of personal and social life are late developments which are sharpened with the rise of reflective and ethical thinking.[8]

A further major result was the definition of the range and extent of the "social heritage": the body of acquisitions resulting, in each generation, from the progressive integration and re-absorption by each individual of all the transmitted culture. This gives a continuous body of accretions (language, institutions, customs, etc.), by a process of social as opposed to physical heredity commonly known as "tradition."

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[6] This identity or interpenetration is strikingly shown in the detailed observations made by Piaget, *Études sur la Logique de l'Enfant,* 2 vols. (also translated into English). The distinction between the self and other persons arises from the fact that these latter are found to exist in both spheres or phenomenal classes, the internal and the external. While themselves centers of inner life, other persons are also recognized as being part of the observer's environment.

[7] In the third edition of *Social and Ethical Interpretations,* the relation of these positons to those of Tarde, Royce, and other writers is brought out. A resumé of the theory is given in the little book, *The Individual and Society,* together with further sociological extensions of the principle.

[8] The student of recent studies of the primitive mind and of early social institutions will have noted the striking support given by them to this theory, which can be read in the light of Lévy Bruhl's theory of "participation." Cf. below, *The Social Factor,* under "Genetic Logic."

III. Evolution

The interest in genesis as such naturally extended itself to the great question of evolution, of which the principles are psychological no less than biological. During the years at Princeton, I made many excursions into this territory, reviewing in various papers such topics as heredity, transmission of acquired characters, the relative importance of endowment and environment, the paralellism between individual development and racial evolution. These, with fuller discussions, were finally developed in the volume *Development and Evolution* (1902). At that date, the two great problems at issue concerned the theory of natural selection, and the possible influence of individual adaptations on the course of evolution. The Darwinians (led by Weismann) were for the moment victorious over those of the Lamarckian camp (Romanes, Eimer, Cope). Among the psychologists in America, Darwinism was in the ascendant, James being one of the convinced converts. The rediscovery of Mendelism had not yet been announced, and the question of mutations was where Darwin had left it in his description of "sports." The Darwinian theory concerned itself, as in the books of Darwin and Wallace, with minute "accidental variations"; and the point of greatest obscurity was that of the seemingly directive or "determinate" course of evolution. The opponents argued for some vital tendency or "directive" factor, represented by the "ortho" in Eimer's theory of "orthogenesis."

Organic Selection. The outcome of my studies was embodied in the position known as "organic selection," printed in the *American Naturalist,* May-June, 1896, and announced also, at about the same time, by H. F. Osborn in America and Lloyd Morgan in England.[9]

---

[9] The original papers of Osborn, Morgan, and E. B. Poulton on the subject were collected, along with my own, in my volume *Development and Evolution.* See also Lloyd Morgan and Weismann in the Cambridge volume *Darwin and Modern Science,* pp. 41 and 428; and consult the bibliography given in my *Dictionary of Philosophy and Psychology, sub verbo.* Searching carefully through the works of Darwin and Wallace, I found only one instance in which the working of the principle of Organic Selection was clearly recognized, namely, by Darwin (see passage quoted *in extenso* in my *Darwin and the Humanities,* American ed., p. 19). Letters on aspects of the topic from various authorities, among them Wallace and Lankester, are printed in my *Between Two Wars,* Vol. II.

According to this point of view, natural selection operating on "spontaneous variations" is sufficient alone to produce determinate evolution (without the inheritance of acquired adaptations or modifications), since—and this is the new point—in each generation variations in the direction of, or "coincident" with, the functon to be developed will favor the organisms possessing them, and their descendants will profit by the accumulation of such variations. Thus the function will gradually come to perfection. In other words, the individual organism's accommodations, made through learning, effort, adaptation, etc., while not physically inherited, still act to supplement or screen the congenital endowment during its incomplete stages, and so give the species time to build up its variations in determinate lines.[10]

From this point of view—that of reinforced Darwinism—the little book *Darwin and the Humanities* was written. It estimates the place of Darwinism in the human sciences—psychology, sociology, ethics, religion—and shows to what extent the principle of natural selection, as reinforced by organic selection, holds good in these subjects.

IV. Genetic Science, Theory of "Genetic Modes"

To one to whom, however, the psychological problem was the central one, the interest in biological evolution was secondary to that in genetic psychology. In the latter, two great problems presented themselves; first, that of method: how can the development of the mental order of phenomena—or that of any other truly genetic order, involving progress—be fruitfully investigated? The Spencerian or quantitative method, brought over into psychology from the exact sciences, physics and chemistry, must be discarded; for its ideal consisted in reducing the more complex to the more simple, the whole to its parts, the later-evolved to the earlier-existent, thus denying or eliminating just the factor which constituted or revealed what was truly genetic. Newer modes of manifestation cannot be stated in atomic terms without doing violence to the more synthetic modes which observation reveals. The qualities of flower and fruit, for example, cannot be accounted for, much less predicted, from the chemical formulas of processes going on in the tissue of the fruit tree.[11]

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[10] Many illustrations of this are given in the works of the authors cited. The application of the principle to the gradual formation and decay of animal instincts is one of the most notable (compare my *Dictionary of Philosophy and Psychology*, article on "Instinct," and the volume *Darwin and the Humanities*, American ed., p. 21).

A method is therefore called for which will take account of this something left "over and above" the quantitative, something which presents new phases as the genetic progression advances. This something reveals itself in a series of qualitative aspects; for example, the empirical qualities of water as such over and above the quantitative and atomic relations given in the chemical formula H^2O. A genetic interpretation requires a formula not exhausted by that of composition or identity (such as water *is*$=H^2O$), but one of genetic progression (such as $H^2O <$*becomes* water), in which, besides the quantitative identification of the H and O, we must further identify the water by *qualities* which were not present in either the H or the O.

The second problem is that of the resulting genetic science, as distinguished from quantitative science; the great body, that is, of data about genesis, development, evolution, which rewards the seeker when the qualitative and other aspects of genetic series, as such, are duly investigated.

These considerations led to the formulation of the "theory of genetic modes"[12] in which the two fundamental positions are:

1) Every truly genetic series is irreversible.

2) Each new stage or term in a truly genetic series is *sui generis* a new mode of presence in what is called reality.

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[11] "Nature achieves novelties; there may be, qualitatively speaking, more or less in the effect than there is in the cause. This position is forced upon us by the radical acceptance of evolution. Spencer tried to subject the whole evolution movement to the mechanical conception of causation; he interpreted all development in terms of successive transformations of energy. Thus life and mind alike were eviscerated of all their richer meaning. So soon, however, as we give genetic change a significance as fundamental as mechanical change, we reach a very different result. Every genetic change ushers in a real advance, a progression on the part of nature to a higher mode of reality. *Actually new things—novelties—are daily achieved in life, mind, and society;* results which we cannot interpret in terms of the mere composition of the elements involved. We cannot predict, for example, the opinions of a group by adding together the convictions of the individuals of the group. Similarly, the outcome of organic growth and of psychological synthesis alike could not be predicted from the most exact knowledge of simple organic or psychic elements, if we did not already know from experience of similar cases, what to expect." (Citation from *Darwin and the Humanities,* American ed., 1909, pp. 86-87).

[12] Published in the *Psychological Review* and reprinted in *Development and Evolution,* Chapter XIX. Also see the paper "The Origin of a Thing and its Nature," Chapter XVIII of the same work.

These two determinations have turned out to be the corner stones of the various theories of "creative" or "emergent"[13] evolution now in vogue.

Put in terms of the formed logic of the case, two postulates were formulated (*Development and Evolution,* pp. 303, 311); first, "The logic of genesis is not expressed in convertible propositions," and, second, "that series of events alone is truly genetic, which cannot be constructed before it has happened, and which cannot be exhausted, by reading backwards, after it has happened."

V. Instrumentalism, Selective Thinking

In the late nineties there was a return in America to problems of an epistemological character. It gave rise to a re-examination of the psychological bases of philosophy. William James took a natural lead in these debates. Truth, error, the method and validity of knowledge became topics of real vitality, and instrumental and pragmatic theories of many varieties saw the light.

From the side of evolution theory, the futility of the older views, which made of thinking an absolute faculty and of truth a sort of psychograph of reality, was evident. The theory of adaptation saw in the rise of thinking a critical turn in the evolution of mind. Knowledge became a function of prime genetic significance, an instrument of supreme utility.

In the Presidential Address read before the American Psychological Association at Cornell University in December, 1897, I examined the process of "Selective Thinking," asking the question how the thinker normally proceeds and what the value is of the results he attains. The result was a theory in which the discovery of truth was recognized as being an adaptation to a given set of data, proceeding by a series of tentative selections from variations of imagery and fragments of hypothetical value. This selection—from hearsay, current half-truths, fragmentary opinions, etc.—is in its method analogous to that of "trial and error" in physical science. Truth is what is selected under the control of the system of established thoughts and facts, and assimilated to the body of socially acquired knowledges and beliefs. Truth thus becomes a tentative and slowly-expanding body of data, more or less adequately reflecting the stable whole of thought and action which is accepted as reality, and in turn enlarging and clarifying that whole.

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[13] The word "emergent" simply expresses by another term the "becomes" or "passes into" of our formula. The sign $<$ used to express this was suggested in my original paper.

In this view, thinking is instrumental in two ways: it is an instrument of adaptive action in an environment of things, persons, and beliefs, and it is an instrument of the clarification and enlarging of the body of accreditated data constituting the system of knowledge, science, tradition, etc., of the race.

This general account of instrumentalism went well with the early statement of pragmatism made by James, and was in line with the point of view later developed in the "Studies in Logic" of the so-called "Chicago School." It stopped short, however, of the pure relativism and subjectivism of many pragmatic writers, inasmuch as it holds that, both as a biological function of trial and error and as an epistemological instrument of scientific and social progress, knowledge presupposes a *dualism of controls*: the agent, on the one hand, and the recognized world of truth and reality—that is, recognized by him—on the other. This was pointed out in the writer's paper, "The Limits of Pragmatism."[14]

VI. GENETIC LOGIC

Method. Broadly stated, the development of mind may be looked at from any one of three main points of view. First, it may be considered *phylogenetically,* as a biological character, and its successive stages traced out in connection with the animal organisms with which it is associated. This gives a biogenetic and comparative research, objective in its method and results.

Secondly, the mind of man taken alone may be investigated in its evolution, and its stages of growth traced in different human races and groups, from the primitive to the most highly cultured. This is the field of *anthropological* research in all its forms (linguistic, social, institutional, etc.). Here again, the method is objective, and the results are at once individual and social. This is a more sober and scientific modern form of the inquiry known as the "philosophy of history," in which history is considered as a manifestation of mind; of this latter, the Hegelian theory is the classical example.

Thirdly, in contrast to these objective methods, biogenetic and anthropological, there is the psychogenetic method properly so-called. Its problem is that of tracing out by the observation of the processes actually going on the essential stages of mental development of the normal human minds taken singly or in groups—the reconstruction of the essential experience by which each individual mind lives, together with its fellows, through its life history from infancy to maturity. This takes us into a research which is mainly subjective, since it must be controlled at every stage by direct individual or social experience.

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[14] *Psychological Review,* 1904, 9, p. 30. It is interesting to note that C. S. Peirce, called the "father of pragmatism," was in agreement with this limitation on the pragmatic point of view.

All of these methods are fruitful and each should be supplemented and corrected by the others. In fact, some of the most interesting formulas so far reached result from correlations of data drawn from biogenesis and anthropology, supplementing the reports of introspective psychology. For example, the law of von Bear, according to which individual development (ontology) recapitulates racial evolution (phylogeny), opens inviting vistas of correspondence between animal and human evolution, on the one hand, and between anthropological and individual development, on the other.

In my work, *Development and Evolution,* the biological theory is utilized in interpreting certain mental functions; in the *History of Psychology* the evolution of individual thought is used to throw light on the course of human speculation about the soul and self; in *Thought and Things or Genetic Logic,* which employs the psychogenetic method, various points have confirmation from sociology and anthropology. For example, the genetic distinction between the prelogical and the logical, reached in Volume I, is directly confirmed by researches in the domain of primitive mentality. The imitative function shows itself equally at work in early social organization and in the development of individual personality. Again, the motive of make-believe or "semblance," also emphasized in that work, is a leading strain in the mythology, folklore, and art of primitive peoples.

The Term Logic. A term had hitherto been lacking to designate the course of organization (whether it be by integration, synthesis, or what-not) by which a given developing function maintains and advances itself. The term "dialectic" was used by Hegel, following Aristotle; Hegel speaks of the absolute as proceeding by a "dialectic" of thesis, antithesis, and synthesis. When, however, the most explicit and evident case to the psychologist, that of thinking, is in question, the word "logic" is commonly used. By a natural extension, this term, logic, may be applied to the processes of mind in general, all recognized as being parts of one continuous movement. This had analogies in expressions already in use, such as "logic of experience," "logic of history," "logic of ethics," etc. Genetic logic was, in my usage, the term adopted to designate the body of inside or psychic processes in which mental development takes place. Within this logic, all the varied special motives of adaptation, opposition, assimilation, etc., uncovered in the detailed researches, show themselves in the panorama of personal and social progress.

Of the four volumes of this work,[15] the division is as follows: Volume I, *Functional Logic,* deals with the prelogical, that is, with operations of mind in the concrete, up to the crisis at which the discursive or thinking processes (logical, in the narrow sense of the word) show themselves. Volume II, *Experimental Logic,* deals with the discursive operations of thought. It is called experimental, because all thinking, as such, is found to proceed by experimental adaptation. Volume III, *Interest and Art,* treats of the development of the active life, its factors being pooled under the concept of Interest, and of the hyper-logical or higher intuitive operations which find their consummation and goal in Art. Volume IV, *Genetic Theory of Reality,* published separately, is devoted to the problem of the natural interpretations of the Real issuing within the movement of experience itself, a series of points of view in which the several motives implicit in the whole of accepted reality (Realism, Idealism, Intuitionism, Aestheticism) take their rise, and to which the mind, in its reflection on itself and the world, naturally resorts.

The Three Stages. The division indicated above is not only convenient for exposition, but the three terms, pre-logical, logical, and hyper-logical, designate well-characterized stages in mental development. They are stages only, not breaks, since the same genetic motives play continuously through these critical points. The mind proceeds, in the *pre-logical* period, by the motives of memory, imagery, play, and action, achieving in its own way the use of general and abstract contents which become "notions" and "concepts," the essential instruments of reasoning. Thus is ushered in the *logical* proper; its essential criterion is the act of judgment. In judgment the presented content of any kind takes form as "schema" or hypothesis and passes from the domain of question or supposition to that of belief. Logical belief, as opposed to primitive and naïve acceptance, is the resolution of doubt, the solution of a problem. Its grounds constitute the "reasons" of formal logic. All the processes of logical mediation—reasoning, argumentation, proof—arise when general and abstract concepts become available for manipulation; successive judgments carry the thinker's belief over a widening system of accepted truths.

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[15] A resumé of the work is given in the second volume of *Between Two Wars,* Chapters XXI and XXII. Genetic logic is there defined (Vol. II, p. 160) as "the research into the principles of the origin and development of mental processes."

This passage into the logical or discursive period brings with it three very striking and fruitful gains. First, language develops *pari passu* with generalization, and gives to all the cognitive and emotional processes the adequate instrument of expression and of personal intercourse. Secondly, the sense of self passes, along with other contents, through various phases of growth, and becomes the "ego" over against the social "alter" (as spoken of again further on). And, thirdly, the rise of judgment brings in reflection, the turning-in of the thinker upon his own mental processes. With reflection, the thinker and agent becomes the judge, the critic, the interpreter, the philosopher.

In the third stage, the *super-logical,* the mind seeks to return to immediacy, to solve the dualism and oppositions inherent in the practical life of thought and action. One or another of the great ideals arises and becomes the place of retreat; and the universal categories of thought, the absolute forms of value, and the various panaceas of feeling erect their claims to final authority.

Results. Accepting this characterization of these great epochs, we may now indicate the leading motives of development which are found to run through them all—passing from perception and memory, through the various phases of the reasoning processes, and finding their consummation in the highest and most subtle of the super-logical, rational, and mystic states of mind.

1) *Semblance.* The function of "make-believe" or semblance is found to have an essential place in mental development. It progresses from the play of childhood, through the imaginative or "schematic"[16] hypotheses used in reasoning, up to the idealizations

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[16] This use of the word "schematic" together with "schematism" is in line with Kant's doctrine of the "schema," a "presentation" or image lying between imagination and judgment. The theory of the "schema" in logic is explained in the article "Knowledge and Imagination." *Psychol. Rev.,* May, 1908.

of art. Semblance has its utility in play, considered as preparation for serious life; in the hypothesis, used as vehicle of the experimental processes of thinking; and in the creative and purifying constructions of art; all for the reason that the semblant images, in all these domains, serve the essential rôle of presenting a meeting place for the two opposing worlds of inner and outer reality. It furnishes the melting pot in which subjective and objective values fuse in an immediacy of direct presence. Here the genetic dualisms between self and the world, between you and me, disappear, and the further world, depicted alike in play, imagination, and art, takes form.

The account of play was based in large measure on the theory developed by Groos,[17] to the effect that in play there is a vicarious or semblant reconstruction of serious situations, serving the purposes of practice and experiment. The same strain I found to be present in all thinking, and also pre-eminently in fine art. The research on this latter point made essential use of the fact of *Einfühlung* or aesthetic sympathy (again referred to further on). But, as is pointed out below, the rôle of semblance is not exhausted in that of practice, as in play, nor in that of emotional relief, as in the Greek theory of the drama, but in the more fundamental fact that it temporarily annuls the hard oppositions and dualisms of actual life, and affords a stage on which reconciling unions and immediacies may establish themselves.

2) *The social factor.* Another of the genetic factors traced out in detail is that of social life or intercourse, as reflected in the individual's growth. The author had already in the earlier work, as indicated above, given attention to the growth of social feeling and conduct in the child, pointing out the elements of give-and-take which react to crystallize, in the actor's mind, the sense of self, alter, and socius, and to establish and develop actual social understanding. In the *Genetic Logic,* the scene is shifted to the inner theater itself. The segmentation and division, so to speak, of the social germ is followed into the great oppositions of personality—dualism between persons and things, that between persons as things, that between persons as thinkers, moral agents, etc. There issues, at a relatively late stage, the hard opposition between the external world, including other persons than the thinker, and the inner or conscious world of the latter—the source of the realism of the

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[17] Groos, K. *Die Spiele der Thiere* and *Die Spiele der Menchen,* both in English translation.

mature life and of positive science. From this dualism of realms of actuality or substance, the thinking individual never afterwards escapes; it is the hurdle in the path of all discursive thinking, as it is the stumbling block to all subjectivist interpretations of the world.

The social or common strain persists in all the discursive processes of thought. All acts of judgment, issuing in verbal propositions, are built upon linguistic elements, by which the content is made communicable. As this proceeds, the judgments take on verbal form which varies both with the thinker by whom they are spoken and with the audience to which they are addressed. They assume typical form in propositions as being conceived *by or for one only* (singular), *by or for many* (particular, "syndoxic"), or *by or for all* (universal, "synnomic"). There is a further logical property to be added to the traditional quantity, quality, relation, modality, namely, what I have called "community" or social reference—the varying meaning of the proposition as being held *by or for different speakers and different audiences.* For example, the exclamation "Great Scott" is held *by me* and *for nobody* in particular; the judgment "you are guilty" is *by me and others who accept it* and *for everyone who hears it;* the statement "lying is immoral" is *by all* (through the moral legislator) and *for all* (as appealing to the common moral sense). This social strain of meaning is shown to hold in all the higher reaches of thought; no proposition whatever, however personal, escapes it.

The implications of this in syllogistic reasoning are brought out in detail. It constitutes, when taken with the establishment of the experimental and linguistic theories of the origin of thinking, a radical revision, for what it may turn out to be worth, of the bases of logic. Instead of a formal dialectic of propositions, thinking is shown to be a vital function, developed under stress of social necessity, in common with its vehicle, language, and preserving, even in its most abstract forms, traces of its instrumental and experimental origin.

"The individual (*Thought and Things,* Vol. II, Chap. 3, Sect. 75) is the result of refined processes of social differentiation. If he makes himself a social unit over against society, he becomes eccentric and anti-social, and his damnation is sure. So of knowledge. It begins common, stays common, claims to be common, enforces its commonness. No knowledge confined to one private head, repeated in other private heads an infinity of times, could ever become an organic system of common knowledge. It must already, in its constitution, reflect its social origin and fitness. The single item of knowledge, the private self-contained thought of a single thinker, is the result of refined processes of cognitive differentiation. The private thought is not a cognitive unit; it is a cognitive outcome. The thought that claims the isolation and absolute lack of common control of an individual unit, is read off as eccentric and unreal, and its damnation is no less sure. Knowledge is common property not a private possession."

Cognitive Mediation. While emphasizing the semblant and social factors of knowledge, it should not be overlooked that its prime mark of difference is mediation of a certain sort. In knowledge one term (fact, image, idea) always stands for, suggests, or leads up to—that is, mediates—another. A memory mediates a fact; the particular case, the general class, the middle term, the valid conclusion. Mediation of truth by fact or idea is the formula of cognitive process. When mediation is absent, as in simple feeling, we recognize some sort of *immediacy*. The various cases of mediation and immediacy are discussed in Volume III, *Interest and Art.*

VII. Affective Logic[18]

Interest. In general we may say that the agent, whether as knower or as doer, finds himself interested in things, both by his curiosities and by his appetites. This, his interest, is set up over against the objects of his knowledge; he takes interest in what he knows and acts interestedly on it. The development of interest presents the great genetic problem of "affective" or "motive" logic.

The forms of interest are very varied. It begins as purely organic response, becomes emotional, turns theoretical, and emerges finally in the complicated modes of sentiment—moral, religious, and aesthetic. There is a real development in this, a very complicated genetic movement, which presents one of the great problems of the future for psychologist and logician.

Looking broadly at the facts, we find that as soon as the object of interest begins to lose its immediacy, as satisfaction of sense or gratification of instinct and impulse, a new method of mediation begins to show itself. The image of memory or fancy serves to suggest the distant pleasure-to-seek or pain-to-avoid. A world of things of desire, things of value, begins to form itself for which the body of facts and knowledges supplies the mediating terms. So the whole system of cognitive meanings—facts, truth, realities—becomes *means* to the pursuit of a further system of values and ends. This *mediation of ends by means* is the method of affective progress. Interest works by using means to secure ends.

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[18] Certain paragraphs of this section follow the longer resumé given in *Between Two Wars,* Vol. II, pp. 166 ff.

Whatever the interest may be, this is the method of its working; even that of thought itself, the theoretical interest. Here the conclusion is the end, and the premises are the means; discovery is the end, facts the means. So we have the entire active life showing itself as a complex system of mediations, where the gains of knowledge or thought become means to further feeling, sentiment, and desire. The entire world of fact or truth is wrapped up in an envelope of value; besides being true, the true becomes useful, good, and beautiful.

Value. In this mediation of ends by means, we have the fundamental formula of Affective Logic and the theory of Value, just as in the corresponding mediation of truths by ideas of facts we have found that of theoretical logic and the theory of truth. The different systems of ends give rise each to its respective system of values. In the domain of knowledge, the ends have theoretical value; in that of personal and social life, the ends have the value of utility, prudence, economy, social prestige, welfare; in the realm of conduct, moral value; in that of beauty and art, aesthetic value. All these form chapters in the very imperfectly developed theory of affective logic.

Affective Revival. In detail, certain conclusions stand out. The theory of "affective memory," based on numerous facts, had been worked out by Ribot and others. According to it, the current view, that only cognitive images or presentations are capable of direct revival, is false. The correlative view, that affective states—emotions, moods, interests—are reinstated only indirectly, when their cognitive objects are reinstated, is also false. On the contrary, there is a direct revival, a reinstatement in memory, of feelings and of affective states in general. This is now clearly established. It has been put especially in evidence in pathological studies of emotion and volition.

This being true, the great question of a "logic" of affective states is opened up. Is there a series of logical processes in the affective life, analogous to those—conception, generalization, abstraction, and proof—recognized in the cognitive life? And if so, what are their principal forms and their rules of procedure?

This general supposition is confirmed in my work; there is a logic of interest and feeling. Besides revival in memory, affective states are subject to comparison, generalization, abstraction, and to vague forms of reasoning, by analogy, substitution, etc. Examples, including affective syllogisms, are given in the work.[19] The great difference, however, between affective and cognitive logic is found in the processes of mediation respectively involved, as is intimated above. Affective logic is a process proceeding by the mediation of ends through means; its result is always in the domain of an interest or value. On the other hand, that of cognition is in the domain of truth. But there are all sorts of criss-crossings and interferences between the two, the processes of truth-seeking rarely being free of influence from the tendencies of feeling and interest which assert themselves when most unexpected. Here the "will-to-believe" shows itself actively, by the intrusion of interest; it finds value at the end of a process which claims to issue solely in the establishment of truth. The mediating image, the middle term, be it cognitive or affective, may be read either as fact, to serve as premise for a conclusion, or as value, to serve as means to an end. The thinker is easily switched by his interest from the mere recognition of the image to the pursuit of the value it holds for him. The so-called neutrality of knowledge is largely mythical; interest and desire give it value which the will is always ready to espouse.

The Great Interests. In the great interests established in the mental life—intellectual (scientific), prudential (economic), moral (and political), religious, aesthetic—the various motives of the development of the self work themselves out. The scientific interest embodies the impulse to know; the prudential interest is rooted in the egoistic motives as such; seeking the gratification of the personal self by the use of social means; the moral interest represents the progress of the ego-alter relation by the idealization of the self-thought as a personal norm and social rule of life; the religious interest seeks the projection of the self in a perfect socius, who is a Companion and Aid; the aesthetic is the interest of reconciliation and unity in the sense brought out below. All of these great interests show the flowering of original and irreducible motives of the active and affective life. They clothe the great human values in social institutions; and none of them is to be denied or replaced.

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[19] *Thought and Things, or Genetic Logic,* Vol. III, *Interest and Art.*

VIII. The Aesthetic Interest

In the domain of what is variously called the life of sentiment, intuition, higher immediacy, etc., the experience of the beautiful, with its correlative impulse to artistic creation, offered until recently, apart from philosophical speculation, an almost virgin field. Art and its enjoyment have always been the domain of very sincere but very indefinite laudation. Certain canons of art, such as those of the "golden section," of unity in variety, of harmony, etc., have been current, and certain superficial characters have been pointed out with varying emphasis: the "symbolism" of art, its truthful or suggestive meaning, its playful and illusional character, its "detachment" from actual fact, its tendency to idealization. But, on the whole, the great masters of art have remained a law unto themselves, and only the result, the successful work of art, has furnished its own criteria and justification.

Aesthetic Sympathy. In the movement toward an empirical psychology in the late nineteenth century, efforts were made by certain observers to find, by actual experiment, the simple proportions and relations which give aesthetic pleasure. But it was in the connection with the rise of affective logic—the determination of the laws of affective revival—that what seemed to be a fruitful point of view was reached. The fact that an artist and, to a less evident degree, the observer of a work of art, in some sense *lives in* or *finds himself involved with* the work of art, had long been noted by artist and spectator alike. "I put my own life into it," says the artist; "I partake of its life," says the spectator. Both identify their own inner movement of feeling with that of the work of art.[20] This was analyzed and expounded by various authors, notably Lipps, and the term *Einfühlung* (translated by "aesthetic sympathy" and "empathy" in English) was given to the general fact.

Semblance in Art. Recognizing the truth of this, and also of the more or less vague requirements spoken of above, the writer found a profitable approach to the aesthetic in the fact of semblance or make-believe, a motive which plays a leading part in certain of the researches already described. The aesthetic experience, whether that of the artist himself or that of the spectator, is found to be a reconstruction of an imaginative and semblant sort. It is analogous to play, akin to hypothesis, involves indulgence in self-illusion, is exercised with freedom from the bonds of actual fact, and fulfills the need of free self-expression and self-fulfillment. Why, it may be asked, does art have this semblant rôle? Why does this sort of indulgence in what is always an artificial construction give the high satisfaction it does?

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[20] This identification of the self with the work of art takes on two forms: the reading *into the object* of one's own feeling or impulse (as the attribution of one's own melancholy to the view of a ruined homestead) and the taking up *into the self* of the feeling or action depicted in the work of art (as in the sense of being taken up by a spire or column or of sympathetic struggling with the victim before such a statue as the Laocoön).

And what relation has the reality revealed in art to the other modes or meanings of reality reported by thought and feeling? Is the Beautiful a successful rival to the True and Good?

The meaning of it in mental development is, I think, clear; and it is extremely interesting. From the start, the growing individual finds himself bound constantly more and more tightly in the bonds of the actual; his actual self makes constant effort and finds constant resistance in the actual world. The two domains, "inner" and "outer," grow harder and more opposed one to the other, as his life adjustments proceed. The dualism of substances grows fixed and rigid. His release from this tension, this very serious business, is found in play, in fancy, in illusion, in fiction—in short, in semblance or make-believe of all kinds. Here he has a sense of freedom, of don't-have-to, of detachment; he plays with symbols, erects fancies, lives the hero, the pauper, the prince, at his own sweet will. In play, as a child or man, he remakes the world, mixing himself with other persons and with things in a delightful chaos; similarly, in art the man and artist again remake the world having in view only his own creation of something—anything—within the possibilities of the ideal reconstruction that the materials allow.

The matter of most importance to the artist is his freedom in the choice of materials over an unrestricted range, but within rules of satisfying construction. In the semblance of play the product is capricious; any old thing will do. There is a make-believe unity. In that of scientific hypothesis, the test and the control are in the domain of fact; what we call truth is what survives. There is a unity of systematization or utility. In art, the attempt is made to return to an emotional and ideal unity, a completeness involving all the various partial motives which the demands of truth and serious living have divorced and made discordant.

Aesthetic Immediacy. In this new immediacy, all values are united. The revival of knowledge is infused with that of feeling; the truth of fact is converted into the value of end; the bond of reality is released in the onrush toward the ideal set up. The self enters to occupy the stage, no longer thwarted by the oppositions of personality or the exigencies of fact.

The preliminaries of this, so far as it is a contribution to aesthetic psychology, were, first, the fact of affective revival, and, secondly, the recognition of the reality of aesthetic sympathy; the positive advance in it is the discovery of the rôle of semblance,[21] as the theater in which the various motives of art reach their fusion. Here alone, it is held, the artist finds the open area, the sphere of immediate presence, in which he may build up by his satisfying thing of beauty. Here, for the spectator, the varied aspects of the art experience, as noted in the literature, fall together in a unique and satisfying synthesis. The thing of beauty matches and surpasses the actual.

IX. Hyper-logical Functions: The Reason

In the third section[22] the attempt is made to give a genetic account of the set of principles generally called "reason," as opposed to reasoning; principles known variously as categories, laws of thought, a priori principles, etc. Kant had made classical the use of the term "reason" in his treatises on Pure and Practical Reason, in which the term *"Vernunft"* designates the a priori or formal, as opposed to the empirical intelligence, or reasoning, indicated by *"Verstand."* Later discussions had brought in more empirical views, especially since the evolution theory had opened the vista of a continuous development of mind in all its faculties. The problem of the origin of the principles of reason had received brilliant treatment in a chapter of James's *Principles of Psychology* where the so-called a priori forms of thought were looked upon as variations empirically hit upon and fixed by selection. Anthropologists were also looking for the rise of these laws of thought or categories in the realm of socially acquired custom, handed down by tradition.[23]

Assuming the validity of this latter position, there remained two questions on which a psychogenetic inquiry such as our own would be expected to have a bearing—questions which would naturally arise in the working out of the motives discovered at work in the earlier stages of mental growth. First, the question as to the actual processes of experience which issue or have issued in the categories or principles of reason; and, secondly, that as to how these principles have become universalized; that is, how have they acquired universal validity apart from concrete experience. This latter resolves itself into the inquiry as to how rules or norms, established as instruments of personal and social life, could be so reflected back as to appear as autonomous reason in the individual. The following answers to these two questions are reached in the work.

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[21] It covers the intimations made by various writers under such terms as "fiction in art," "mensonge d'art," "self-illusion," etc.

[22] Of *Thought and Things*, Vol. III.

[23] A sociological theory carried out strikingly later on by Durkheim and others of the French neo-positivist school.

Reason. It is found that the distinction between the principles of "pure" or theoretical reason, on the one hand, and those of "practical" reason, on the other hand, rests on differences in the processes of mediation which they respectively involve. Among the first named, that is, the theoretical, there are causality, identity, sufficient reason—in short, all the categories or presuppositions of thought; under the latter or practical, there are the norms of conduct—obligation, utility, value. These two great modes of function, cognitive and active, both proceed by mediation, but with a difference already noted above. In the realm of knowledge or reasoning, a given image, term, or concept mediates another; a memory recalls a fact, a face, a distant scene. Here it is all within the domain of knowledges or cognitive meanings. In the realm of action, on the contrary, the distant term, the experience mediated, is an end—a satisfaction, a realization, a value—set up as object of desire; and the mediating term is the means used to attain that end. For example, a dollar, let us say, mediates a dinner, both in theory and in practice; in theory, because in my thought the dollar is convertible into food; in practice, because I can plan the menu and order the dinner which I get for the dollar. In the complex development of scientific and symbolic thought, all sorts of abbreviations, substitutions, and shuffling of terms occur; but the conclusion is always a restatement, in more or less sublimated form, of the same terms. And in the active life, the sciences of economics and ethics are built up on the successive stages in the supposed mediation of individual and social values considered as personal ends.

As the mind grows into the superlogical stage, these processes become in both cases typical and general, but with a very curious difference in the outcome. On the logical side, the scaffolding of mediation becomes itself a universal instrument, apart from its content of concrete images or concepts: the syllogistic forms come to have an independent or a priori force, and pure thought emerges—thought, that is, which thinks of anything or nothing. The subject of thought has fallen out, leaving the shell or form. In the practical realm, on the contrary, it is the apparatus of mediation which falls away, while the specific end set up assumes absolute value as the good, the beautiful, the true. While in the theoretical, the process drowns the content—the process remaining the same whatever the content—and the content, being singular as value and personal as end, survives the form. The scaffolding of mediation falls away and the end reveals itself as a supreme and ideal value. In this we see depicted the passage from the empirical of personal and social life, to the universal of rational form. The socially established and mediate takes on the form, on the one hand, of an immediate datum of reason, and on the other, of an absolute value.

X. INTERPRETATIONS: PANCALISM

In the higher reaches of mental development, the thinker attains, in the normal life of thought, understandings of himself and the world which confirm or modify variously his naïve acceptances and beliefs. It becomes then a legitimate problem to determine the types of interpretation that the mind, both individual and social, puts upon its own products—its truths, its values, in general its "realities."[24] Does it accept as final its own natural dualism of self and the world (remaining) or does it reduce one of these terms to the other (becoming idealist)? Does it deny the rôle of reflection (becoming positivist) or attempt to escape the claim of thought (lapsing into mysticism)? Does it finally appeal to something outside itself for light and leading (finding Religion the absolute organ of reality)?

The individual falls on occasion into each of these interpretations, following his temperament, training, or the example of others; and the race does likewise, both naturally in its institutions, and reflectively in its philosophy. The great institutions of human progress—scientific, economic, religious, artistic—each rests on one of these motives and builds itself upon it, as if it possessed and could reveal the whole truth. The philosophic thinker, in his turn, seeks some one motive to unify this rich heritage, while conserving all its elements—all the fine accretions to life and thought that the race has acquired by toil and sacrifice. What, he asks, is at the bottom of it all? What experience reveals the richest synthesis and indicates the most satisfying presence of reality—giving to each of the partial and seemingly equal "real" things of thought, desire, and feeling, its proper place and value?

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[24] This is the general problem treated in the concluding volume of the *Genetic Logic* entitled *Genetic Theory of Reality*.

The faults of the historical interpretations of reality are brought out in detail. The theories are classified under the headings of Intellectualist, Voluntarist, and Affectivist. The Intellectualist theories—all the rationalisms, realisms, and actualisms which start out from cognitive data of fact or truth—leave unsolved the dualism between the world of truth established by thought, on the one hand, and that of value (the satisfying, the desirable, the ideal), on the other. Voluntarist theories, whether moralist or pragmatic, placing the final emphasis on value, give no proper place to truth as such. Thought, in these latter theories, loses its autonomy as instrument of action. In the two great divisions thus characterized, we see the two great types of mediation pushed respectively to the front: that of "fact by idea" and that of "ends by means." One or the other has its apotheosis, while the other is made subordinate. But in fact both always survive and the opposition remains to the end.

The Affectivist theories have had little development. They include the immediatisms and mysticisms of all sorts. Religious mysticism is its most important historical form. The religious interpretation of reality fails to solve the dualism between the finite and the infinite personality, as well as that between the self and the world. The religious appeal to God evidences the sense of personal isolation and confirms the futility of the individual scheme of life; it affords, besides, only a personal and fleeting reconciliation. The ecstacy of absorption in God of religious mysticism attains its end by the loss of personality in a mystic union which in proportion as it succeeds becomes an empty and meaningless Nirvana.

The Aesthetic interpretation is that to which one is led in carrying further the research into the nature and rôle of the aesthetic interest as characterized above.

The mind itself, seeking spontaneously a way of reconciliation of its realities and values among themselves, resorts, as has been seen, to the sort of artifice found in the general function of "semblance." In play, in reverie, in imagination, in hypothesis, in mystic absorption, in each of these the agent escapes the immediate struggle and the urgent task by indulging in "self-illusion." He creates in semblance a complete and harmonious reality, forming in turn a play-world, a stage-world, a world of spirits, a "city of God," where, for the moment at least, he finds both peace and freedom. But in art alone does this sort of construction lose its temporary and capricious character and take on permanent and progressive form. In the aesthetic semblance of fine art we find a permanent mode of reconciliation which includes all the serious factors of life and welds them into a full and satisfying intuition of reality. The thinker finds himself a "pancalist," as does the present writer who carries out this interpretation in a philosophical theory of reality called Pancalism.[25]

Aesthetic Reason. There is, in short, a third sort of "reason" to be added to the two known as theoretical and practical, if we continue to use the old word "reason." Kant clearly stated this problem from the point of view of his *Critique*.[26] In the light of what we have found concerning the origin of the theoretical forms and practical rules, each revealing a return to its own experience of immediacy, we see what the aesthetic reason is. It is the immediacy of reconciliation in which the true and the good being reconciled in a semblant scheme, the agent reads into their union a charge of personal sentiment and value. The artist's feeling absorbs and reinterprets what it depicts. The norms of this construction, that is the formal elements which best satisfy the thinker and advance the construction, constitute the "aesthetic reason." They signalize the conquest of truth and goodness by the sentiment involved in the achievement of beauty. The function of art criticism is to re-decompose what aesthetic intuition has composed and to reveal at once the rules of valid art and the norms of its appreciation.

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[25] The view that in aesthetic intuition, as exercised in the contemplation of a work of art, there is the experiential basis for a philosophical theory which escapes the criticisms of partiality and exclusion, briefly referred to above, to which the traditional alternatives of idealism, voluntarism, personalism, etc., are exposed. It is by recognizing the motives valid in each and all of these partial views, and by following the example of the spontaneous conscious process itself, that the truly synthetic principle is found in the realm of Art. This view is developed in the work cited; its motto is the Greek *τὸ καλὸν π αν·*

[26] His *Kritik der Urtheilskraft* was an attempt to find the a priori forms of sentiment, aesthetic and other, analogous to the "categories" of thought and the "imperatives" of conduct.

XI. Terminology and Editorial Work

At an early day, I was impressed by the difficulty of profitable discussion in the newer branches of psychology, by reason of the paucity and ambiguity of the terms in use. The older discussions, dominated by theological and ontological conceptions, suggested terms of metaphysical bearing, such as soul, reason, cause, creation, vital force, etc., while the new researches into genesis, social experience, etc., required close distinction and exact definition. The more literary writers, citing Emerson and James, held that psychology should be sufficiently clear to express itself in terms familiar to the uninitiated. This was and is the ideal in France. On the other hand, there were many in America, among them notably C. S. Peirce, advocating his views in the New York *Nation,* who proposed to cut loose entirely from popular usage and coin a clear and consistent terminology for the mental and moral sciences as had been done for mathematics and symbolic logic. While not going the whole way with the latter, I was convinced that confusion lurked in most of the discussions of the day, from the lack of well-defined terms; and in the *Genetic Logic* I suggested certain new terms found necessary here and there as the work proceeded, of which a glossary is appended to the fourth volume.[27]

As a step toward reform and common understanding in the matter, the project of a work of reference, a dictionary or cyclopedia, took form, in which terms in use in all the psychological and moral sciences should be defined and new terms already suggested here and there duly passed upon. The *Dictionary of Psychology and Philosophy* appeared 1901-1906. It combined encyclopedic with lexicographical features, the work of over sixty collaborators, principally in the United States and England. Besides establishing an exact usage under each term treated, it also suggested equivalents in three other languages.[28]

Another project in the public domain, so to speak, was that of the *Psychological Review* founded jointly with J. M. Cattell in 1895. Its fruitful career and subsequent material enlargement testified to its real function in stimulating psychology at home and abroad. It provided the fortnightly *Psychological Bulletin* for shorter articles, and the *Monograph Supplements* for long treatises, in addition to the regular bi-monthly issues, and founded also the *Psychological Index,* an annual catalogue of publications the world over.[29]

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[27] *Genetic Theory of Reality, ad fin.*

[28] Some of the vicissitudes of the project as well as those of the editing of the *Psychological Review,* amusing no less than serious, are related in the volume of memoirs *Between Two Wars,* Vol. I, pp. 71 ff.

Other interests, also in the public domain, were concerned with the progress of psychology in America and abroad: reports on psychology for the Chicago and St. Louis Expositions, report of special advisory committee of the Carnegie Institution on the needs of research (*Bulletin of the Carnegie Institution,* Vol. I), foundation with J. McBride Sterrett and others of the Southern Society of Philosophy and Psychology (1905).

XII. PRACTICAL STUDIES

The years 1914-1924, which might have been given over to close psychological work, were, on the contrary, absorbed by the interest and excitement of the World War and its settlement. Involved from the start through engagements in Paris and possessed of strong opinions on the questions at issue, my literary energies took form in what may be described as comparative national and political studies. Publications in book form were: *The Super-State* (Herbert Spencer Lecture, Oxford, 1916), *French and American Ideals* (1914), *France and the War* (1916), *American Neutrality* (1916), and a collection of papers and addresses.[30] All this reflected an intense absorption in practical interests.

Purely theoretical interest in problems of knowledge, time, space, art, philosophy, suffered an eclipse everywhere, and in my own case it was very slow in re-emerging. The person of thought had become the man of action; the problems of national ethics and juridical reconstruction crowded to the wall the more sober inquiries as to the "why" of the universe or the "how" of evolution. No doubt most of the men engaged in meditative studies before the war suffered more or less from this cataclysm of personal interest, this inrush of the practical to the extinction of the theoretical. War, death, shame, glory, these calls of the blood once listened to, the energies of life flow unrestrained. Questions of social right and wrong take the forms induced by acts of aggression and violence; and attitudes of criticism give place to demands for sanction, punishment, and reparation. What the world lost in reflective thought in losing a generation of thinkers by death, prostration, and emotional obsession, will, of course, never be known. But with it all, I, for one, do not envy the men who held themselves above the mêlée or took the rôle of objectors, whether "conscientious" or prudential, during the play of the gigantic moral forces that clashed in those fateful years.

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[29] In these supplementary publications, the *Psychological Review* was pioneer; but the review proper was second to the *American Journal of Psychology,* founded several years earlier.

[30] Collected in the volume *Paroles de Guerre d'un Americain* (in French, 1916) and reprinted in *Between Two Wars,* Vol. II.

XIII. ESTIMATIONS

Casting a glance backward over the course of psychology in the last generation, one sees the rise and fall of certain tendencies. Besides the genetic and social motives dwelt upon above, which have continued to progress, there have been other marked interests. Certain of the newer problems have been those of individual endowment and capacity, studied through mental tests (the United States and France); the application in practice of these differential studies (United States); the study of the unconscious, especially in application to the abnormal through psychoanalysis (Austria, Germany, United States); the objective study of mind both in its social evolution (France) and in its positive reaction in behavior (United States); statistical studies of child development (Switzerland). Of these the most promising, in my opinion, are those of the new sociology of the so-called Durkheim school in France, and the child study movement in Switzerland centered in the J. J. Rousseau Institute and in the work of the group led by Piaget. Of the larger standing problems, those of the affective life in general, indicated above under "affective logic," offer greatest rewards to the future psychologist.

The psychoanalytical movement has about spent itself, after a career of popular and unscientific propaganda, notably in the United States. Based on sometimes unreal and always extravagant presuppositions, as in the theory of the libido and in the interpretation of dreams, Freudism, nevertheless, is an instrument of some value when divorced from the applications made of it by the parlor psychologist and the charlatan. But its great defect is its shifting foundation; it rests on a morass. Results are reached showing that any symptom or character may be due equally well to the absence or presence of one or the other or both of two contradictory motives, repression and expansion, the sources of which are, fortunately for the psychoanalyst, too obscure to be subjected to examination. An individual is aggressive either because he simply is aggressive or because, being weak, he acts to expand himself in a way to cover his weakness; a character is modest either because he really is modest or because, being vain, he strives to camouflage his vanity with a covering of modesty. Alexander was militant because he was physically imposing; Wilhelm II was militant, because, having a withered arm, he must react to play the war-lord. Such are the cheap resources of psychoanalysis. Commonplaces are drawn from the profound obscurities of the subconscious. The place of sound hypothesis is too often taken by wild analogy such as those drawn from sex, and instead of sober scientific interpretations we have fanciful inferences seen at their climax in the "Oedipus complex" and in the interpretation of dreams.

The main facts of the existence of repressed impulses, of traumatism in the emotional subconscious, of release by suggestion, and of defense by "sublimation," utilized by the psychoanalysis, were established by "analytical" psychology under other and often better terms, before the appearance of Freud. The clamor made over originality is, it must be said, mainly over an originality of terms and pretensions.

Another theory popular in America, the country of intellectual fads and the worship of new words, is that of "behaviorism." It is a refined and, in itself, valuable recourse to the objective method proper to physiology and biology, of which, in fact, it forms a legitimate chapter. It carries further, on the positive side, the "motor" and experimental studies of the earlier generation. But it is not psychology; it is biology, and, at the best, physiology. To be available to the psychologist, its results must be interpreted by the introspection of the reagent; for none of the results of the method could be applied in psychology if we did not already know from experience the conscious connotation of the terms used. The same surgical operation, for example, made with and without anaesthetics, shows the absence or presence respectively of pain, a conscious state. The psychological difference consists just in the presence of pain. The behaviorist asserts that there are also subtle differences on the side of brain and nerve, that is to say, in behavior. Agreed; but of the two reactions which, we ask, is the one belonging to or accompanied by the pain? This is what consciousness alone can determine, for to know this the reagent must have the pain. What mode of consciousness goes with this or that organic reaction? Just here arises the series of questions that psychology has always put and must continue to put in vain to physiology. The discussion is a very old one, dating from August Comte and Huxley and continued by all those who have claimed to carry by assault the citadel of self-conscious experience; but in fact there is no drawbridge across the moat. *A fortiori,* all the familiar forms of logical and reflective experience—the presence of values, ideals, spiritual interests and aspirations—all disappear, disowned by the behaviorist who, becoming amateur philosopher, revamps the worn-out formulas of materialism.

XIV. Resumé of Results

The editors of this work especially ask the writers for a show of preference, a selection, from the things they have done, of what is of relative importance and interest. But for this request such an appreciation might seem unbecoming. At any rate, a traveller, on looking back over his course, may be able to point out where, in his opinion, the path has been straight and smooth; and by reason of his age and experience, his estimation may serve somewhat to direct the oncoming recruits. Furthermore, it is understood that in selecting among his own children he makes no comparison with those of others and in no way sets up standards of comparative value.

The things I value relatively in this sense are: first, the genetic *Method* pursued and, secondly, the *Results* acquired in genetic and social Psychology and Philosophy. These results may be briefly summarized as follows:

1) *General and Experimental*: Child Study results; Imitation and Circular Reaction; Motor Interpretations generally.

2) *Evolution*: The theory of Organic Selection; the theory of Genetic Modes, as serving as basis of Genetic Science and of General Evolution.

3) *Social Psychology*: The social origin of the Self; the Correlation between personal and social growth through the processes of Imitative Assimilation and social "give-and-take."

4) *Genetic Logic*: Place of Semblance and "Schematism" in mental development; tracing of the "common" element in knowledge, the doctrine of logical "community"; "Instrumentalism" of knowledge and thought; development of Affective Logic and the theory of Value; Social and Instrumental derivation of the forms of Reason.

5) *Aesthetic Psychology*: Nature of Art Appreciation; the place in philosophy of Aesthetic Intuition (Pancalism).

MARY WHITON CALKINS*

I

1) I began the serious study of psychology with William James. Most unhappily for them and most fortunately for me the other members of his seminary in psychology dropped away in the early weeks of the fall of 1890; and James and I were left not, as in Garfield's vision of Mark Hopkins and himself, at either end of a log but quite literally at either side of a library fire. The *Principles of Psychology* was warm from the press; and my absorbed study of those brilliant, erudite, and provocative volumes, as interpreted by their writer, was my introduction to psychology. What I gained from the written page, and even more from tête-à-tête discussion was, it seems to me as I look back upon it, beyond all else, a vivid sense of the concreteness of psychology and of the immediate reality of "finite individual minds" with their "thoughts and feelings." James's vituperation of the "psychologist's fallacy"—the "confusion of his own standpoint with that of the mental fact about which he is making his report"—results directly from this view of introspection as immediate experience and not mere inference from experience. From introspection he derives the materials for psychology. "Introspective observation," he expressly asserts, "is what we have to rely on first and foremost and always. . . ."[1]

Of specific doctrines, those which I now recall as most impressing me, in this early study of the *Principles,* are the criticisms levelled against the conception of "Unconscious Thought" and against automatism; the nativistic space doctrine; the emotion theory; the reiterated teaching (obviously an anticipation of the *Gestaltpsychologie*) that a percept has a unity of its own and is no mere aggregate of sensations; and the emphasized conception of consciousness as in its very nature impulsive. More significant, as events proved, for my own system of psychology are the doctrine of the transitive feelings of relation, the feelings of *and, if,* and *but,*[2] and the concept of consciousness as tending to the "personal form."[3] The truth is, however, that each chapter of this incomparable treatise left some impress on my mind so that, to this day, I can turn with assurance to the chapter and page in which James considers this or that topic.

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*Died February 26, 1930.

[1] *Principles of Psychology,* Vol. I, pp. vi, 196, 185[2].

[2] *Op. cit.,* I, pp. 345 ff.

[3] *Op. cit.,* I, pp. 225 ff.

I was equally fortunate, in this same fall of 1890, in entering on laboratory work under the guidance of Edmund Sanford, a teacher unrivalled for the richness and precision of his knowledge of experimental procedure and for the prodigality with which he lavished time and interest upon his students. Besides training me in the detail of laboratory experiments, Dr. Sanford started me upon a "minor" research problem, based on the records which, during seven weeks, he took of his dreams and I of mine. The study of these records constituted in itself a course in general psychology from the vantage ground of a systematic introspection of these dream phenomena and with the constant stimulus of Dr. Sanford's suggestion. The distinguishing features of the study were these: We, the observers, waked ourselves (by the use of alarm-clocks) at different hours of the night; we recorded our dreams at the instant of waking and each morning studied with care all the records, whether slight and trivial or seemingly significant. We took account of the different types of dream experience, discovering elements of all sense modes, emotions of every sort, and occasional examples of dream reasoning and dream volition; and we considered also the relation of the dream to the waking life, distinguishing in particular the persons and the places of our dream experiences. The conclusion which I reached, that the dream merely reproduces "in general the persons, places and events of recent sense perception" and that the dream is rarely "associated with that which is of paramount significance in one's waking experience,"[4] is almost ludicrously opposed to the nowadays widely accepted Freudian conception of the dream; in fact, my study as a whole must be rather contemptuously set down by any good Freudian as superficially concerned with the mere "manifest content" of the dream. It is, however, of interest to me to notice that my old dream study does anticipate more than one of the findings of the psychoanalysts. In agreement with them, for example, it vigorously disputes the assertions of people who report that they never dream; and this on strictly empirical grounds. For I had more than one instance of waking without the faintest memory of having dreamed and of discovering by my side the night record of one dream or of several.[5]

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[4] "Statistics of dreams," *Amer. J. Psychol.*, 1893, **5**, 334, 332[3].

[5] Cf. Brill, A. A. "Fundamental conceptions of psychoanalysis"; "Everybody dreams" (p. 140). Other agreements are the following: (1) "The mind takes a problem and works it into a dream" (Brill, *op. cit.*, p. 148)—a suggestion of the reasoning dream discussed in my "Statistics of dreams" (*op. cit.*, p. 325); (2) "it is invariably something of the day before the

A second fruit of this first year of graduate work in psychology was a paper on association which I wrote for Dr. James. I had first proposed 'attention' as my topic, but he frowned on this (if I rightly remember) for the highly characteristic reason that he was sick of the subject. Quite at random I next chose 'association,' thus determining my chief interest for a number of years. This paper turned out to be my first published contribution to psychology. It appeared, suitably condensed, in an early issue, July, 1892, of the *Philosophical Review.* The paper takes its start in the conception of association as observable connection between succeeding objects (or contents) of consciousness; proceeds, after James's fashion, to reduce so-called association by similarity to contiguity association; and is largely concerned with a classification in which, modifying that of James, it lays stress on what it calls the persisting element in cases of 'multiple' and 'focalized' partial association. I can hardly hope ever again to be so puffed with pride as when I found this distinction approvingly referred to in a footnote of the second edition of "little James," the name by which, at this time, we all knew the *Briefer Course in Psychology.*

Chronologically third of my great teachers in psychology was Hugo Münsterberg, a man of deep learning, high originality, and astounding versatility, interested alike in systematic psychology, in the setting and solution of experimental problems, and, years later, in the applications of psychology. In the very fall of 1892, when I had planned to ask admission to his Freiburg Laboratory, he came instead to Harvard; and for parts of three years I worked under his inspiring direction in the old Psychology Laboratory of Dane Hall. The Laboratory was infelicitously situated within hearing on the one side of the hand-organs and the street-car bells of Harvard Square and on the other of the often vociferous outbursts of Professor Copeland's "elocution" classes, but it was none the less the scene of absorbing work. I shall not let this opportunity pass by to record my gratitude for the friendly, comradely, and refreshingly matter-of-fact welcome which I received from the men working in

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dream that starts the trends of the associations" (Brill, *op. cit.*, p. 241[2])—a statement closely resembling my conclusion that "the dream is connected.... in the experience of these observers....with the recent life" (*op. cit.*, p. 331[3]); (3) "a quotation in the dream is always based on something seen or read but it is usually modified to fit the situation in the dream" (Brill, *op. cit.*, p. 244[3])—an accurate description of more than one of what I called my verbal dreams (*op. cit.*, pp. 322 f.)

the Laboratory as assistants and students, by whom the unprecedented incursion of a woman might well have been resented. My abiding gratitude to Dr. Münsterberg, who swung the Laboratory doors open to me, is supplemented by my appreciative memory of Edgar Pierce and Arthur Pierce, of Robert MacDougall and James Lough—to name no others—who, throughout these years, were my mechanicians, subjects, counsellers, and friends.

I interrupt myself to interpolate a frivolous reminiscence, of a much later date, which sets off in bold relief the friendly tolerance of my Harvard fellow-students. I was a member in 1905 of the Executive Committee of the American Psychological Association. Dr. Münsterberg had planned a lunch-meeting of the Committee at the Harvard Union, but the burly head-waiter stoutly protested our entrance. No woman, he correctly insisted, might set foot in the main hall; nor was it possible to admit so many men, balanced by one woman only, to the ladies' dining-room. It was almost by main force that Professor Münsterberg gained his point and the Committee its lunch.

My problem for experimental investigation was a comparison of frequency, recency, and vividness as conditions of association. In brief, I showed that, in direct competition, recency yields to vividness, and both vividness and recency to frequency. Concretely stated—in showing series of colors paired with numerals I found that a numeral which had repeatedly appeared in conjunction with a given color was more likely than either a vividly colored numeral or than the numeral last paired with the color, to be remembered, on a reappearance of the given color. Perhaps more significant than these results is the method, since known as that of right associates, which I employed. For I discovered presently, to my unbounded surprise, that I had originated a technical memorizing method. G. E. Müller, who sharply criticized and greatly refined, but in essence adopted the method, calls it the *Treffermethode;* Titchener paid the experiment the high compliment of including it in his *Students' Manual;* and, only a year or two ago, Professor Kline selected it as one of the exercises in his *Psychology by Experiment.* I have strayed so far from the path of experimental procedure, while consistently placing so high a value on the experimental method, that I take unaffected pleasure in the thought of my one slightly significant contribution to experimental psychology.

My work in association, theoretical and experimental, was brought together in a monograph published in 1896 (the second of the *Psychological Review Monograph Supplements*) and would have constituted my doctor's thesis had the Harvard Corporation approved the recommendation of the Department of Philosophy and Psychology to grant me a doctor's degree. My natural regret at the action of the Corporation has never clouded my gratitude for the incomparably greater boon which they granted me—that of working in the seminaries and the laboratory of the great Harvard teachers. My debt, both academic and personal, to these men, to James, Royce, Palmer, and Münsterberg, may be acknowledged but can never be repaid.

Meantime I had begun my teaching of psychology. Officially, it was I who had the honor of setting up at Wellesley, in the wide attic spaces of the fifth floor of old College Hall, one of the earlier (and smaller) of American psychological laboratories. Actually, the laboratory was the creation of Professor Sanford, whose counsel I sought and received in large things and small, in planning the expenditure of my restricted laboratory fund, in placing orders with European apparatus makers, and in the selection and purchase of materials nearer at hand. Several pieces of apparatus were made from Dr. Sanford's specifications by Wellesley carpenters; our chronoscope (one of his own invention), our Wheatstone stereoscope, and other pieces were constructed by a Clark University mechanician. The fire of 1914 destroyed apparatus and laboratory, but the workers today in the Wellesley Laboratory gratefully acknowledge Edmund Sanford as its founder.

Looking back on these earlier years of psychology teaching, I seem to myself to have gained three useful, though disparate, ends. In the first place, I "held the fort" for my successor in the direction of the Laboratory, Dr. Eleanor Gamble, an experimentalist far better endowed and equipped than I.

I had the opportunity, in the second place, to conduct among some hundreds of students an investigation of the prevalence, nature, and types of synaesthesia and mental forms.[6] In each of the years 1893 and 1894, the entire freshman class was canvassed through questionnaires supplemented by personal interviews; and the cases of synaesthesia reported in 1893 were found, by subsequent unannounced questioning, to persist, with one exception, through months and often through the year.

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[6] *Amer. J. Psychol.*, 1893, **5**, 439 ff.; 1895, **7**, 90 ff.

In the third place, I worked out a course in general psychology in which simple experiments provided first-hand material for the study of a number of topics. A paper, written at the invitation of President Hall, and published during 1893 in the *American Journal of Psychology,* briefly describes this rather crude course. More or less external conditions greatly modified it, with the years, but I take this opportunity to register my ardent championship of an inductive method in introductory psychology courses. I am convinced that exercises in introspection, whenever possible experimentally controlled, should precede both the reading of textbooks and the hearing of lectures.

2) Time would fail me, and interest would certainly fail my readers, were I to dilate on all the topics of psychology for which I have felt a special concern. The list would include color-theory,[7] the criterion of animal consciousness,[8] the analysis of the space-consciousness,[9] and the theory of the "physiological correlate of emotion."[10] But though I pass with bare mention subjects such as these, I must treat more respectfully four major interests of my first decade of work in psychology—interests which still persist. These are: the study of association; the conception of the psychic element; the doctrine of relational elements of experience; finally, and most important, the conception of psychology as science of self with which I contrasted atomistic or idea-psychology, the study without reference to any self, of successive experiences. Both conceptions of psychology, I maintained, are valid and useful; but I deprecated strongly the tendency of psychologists to alternate irresponsibly between one and the other.[11]

Two papers which I published in 1900 gather up between them my convictions on all four of these subjects of my main interest and serve as a sort of program for the work which followed. The earlier of these papers is entitled "Elements of Conscious Complexes"[12] and is mainly concerned with psychology from the atomistic standpoint.

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[7] "Theorien über die Empfindung farbiger und farbloser Lichter." *Arch. f. Anat. u. Physiol.* (*Physiolog. Abtheilung. Supplement*), 1902.

[8] "The limits of genetic and of comparative psychology." (A paper read at the International Congress of Arts and Science, St. Louis, 1904.) *Brit. J. Psychol.,* 1905, **1,** 262-285.

[10] *Ibid.,* pp. 207 ff. Cf. *An Introduction to Psychology,* 1901, pp. 114 ff.

[9] *A First Book in Psychology,* 1910, 1914, Appendix, Section IV., pp. 336 ff.

[11] "Elements of conscious complexes." *Psychol. Rev.,* 1900, **2,** p. 479.

[12] Cited in the preceding note.

Its theory of consciousness as a succession of experiences or ideas is obviously closely related to conceptions underlying my study of association, and was probably influenced also by Titchener whose *Outline* and *Primer* I was using in my classes. In addition to its stress on this conception of psychology, the paper has two main emphases: in the first place, it seeks to replace the doctrine that psychic elements have attributes by the more rigid conception of the so-called attributes —the sensational intensities and extensities, for example—as themselves psychic elements;[13] in the second place, it takes up the cudgels for the James and Spencer conception of relational or thought elements. The first of these doctrines still commands my firm adherence, but I have long since ceased bickering about it for it now seems to me relatively unimportant. Anti-sensationalism, on the other hand, is to this day a live issue; and I am as much concerned now as I was in 1900 to affirm the unsensational nature of such experiences as the consciousness of the likeness of one color to another. To the fruitful experimental investigation of these thought factors in experience by Woodworth, Bühler, and others, my colleague, Eleanor Gamble, and I, a few years later, made a small contribution by repeating with modifications two investigations of Alfred Lehmann and, we believed, proving in opposition to his conclusions that recognition does not consist in reproduced images and that neither the consciousness of likeness nor that of difference is constituted by a verbal image.[14]

The second of the program-papers, published in 1900, considers psychology as science of self. It is the first systematic statement of my self-psychology but by no means the earliest indication of my interest in the 'self.' Before I summarize this article I shall turn back, therefore, for references to the self in my very first psychological paper and in two others of the nineties. A "presupposition of the fact of association," I wrote in 1892, "is that of the identity of the subject. The same 'I' must exist if there is to be consciousness 'in the same way' or 'of the same object.'" To this statement I added a sentence which, from my present standpoint, I should vigorously blue-pencil: "A discussion of the nature of this 'I' would be an unwarrantable intrusion of metaphysics into psychology."[15] After the same fashion, in the monograph published four years later, I said that "a continuous self seems to the writer to be an inevitable presupposition of psychic phenomena of every kind" and again added that this presupposition of the self "leads us at once from the matter-of-fact plane of psychology into the domain of metaphysics."[16] A third and still earlier instance of my concern for the self is found in the paragraphs of my "dreams paper" which discuss the alleged loss of personal identity in dreaming. "The loss of identity in dreams," I wrote, "is not a loss but a change or a doubling of self consciousness. . . . Yet all the time one is conscious that *it is oneself* who has changed or whose identity is doubled."[17]

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[13] Cf. my paper, "Attributes of sensation." *Psychol. Rev.*, 1899, **6**, 506-514.

[14] *Zsch. f. Psychol. u. Physiol.*, 1903, **32**, 177-199; **33**, 161-170.

[15] "A suggested classification of cases of association." *Phil. Rev.*, 1892, **1**, p. 392[1].

From this digression I return to the paper, published in 1900, on "Psychology as Science of Selves." Here, I once and for all renounced "the misleading treatment of the self as metaphysical presupposition" and maintained that selves "may be treated as facts for science," since "they are taken for granted without inquiry about their bearing on 'reality,' and. . . . are critically observed and classified on the basis of their relation with each other and with facts of every other order."[18] In accordance with this doctrine, I described selves as fundamental phenomena, basal to what I called "facts for selves," namely, "contents of consciousness," on the one hand, and physical things and events, on the other. Atomistic psychology I still recognized as a valid science concerned with these psychic events called contents of consciousness. The psychology of selves, on the other hand, I conceived as "frankly" acknowledging the contents of consciousness as experiences of some self and proceeding to the study of these selves "in their diverse relations to each other and to facts of other sorts."[19] Perception, for example, I described as "a consciousness of sharing the experience of a number of other selves," as opposed to "our unshared individual experience" in imagination; and I contrasted the "passivity of the emotional experience with the activity of 'will' and of 'faith.' "[20]

I wish that I could recall more completely the sources of this personalistic doctrine of psychology. In my emphasis on the social nature of the self, I was certainly influenced by Baldwin and by Royce, to both of whom I explicitly referred. I am confident, also, despite a lack of external evidence, that my self-doctrine must have been affected both by the earlier part of James's chapter on "The Stream of Consciousness" and by Ward's famous "Psychology" article in the *Encyclopaedia Britannica,* a treatise which probes to the very heart of every topic which it considers. "Everything experienced," he here says, "is referred to a self experiencing." My conception, finally, of the double standpoint in psychology, the theory that every experience may be treated alike from the atomistic and from the self-psychological standpoint, was certainly influenced by a doctrine from which, none the less, it markedly differs—namely, by Münsterberg's distinction between (1) psychology as science of causally connected complexes of psychic elements and (2) history, described as science of "real subjective will-acts," or will-attitudes.[21]

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[16] "Association." *Psychol. Rev., Monog. Suppl.*, 1896, **2**, 4-5.

[17] "Statistics of dreams." *Amer. J. Psychol.*, 1893, **5**, 335^{3}-336.

[18] *Phil. Rev.*, 1900, **9**, pp. 491^{3}f.

[19] *Ibid.*, p. 501.

[20] *Ibid.*, pp. 489-499.

To Dr. Münsterberg I submitted early drafts of the papers, which the preceding pages have summarized, as working plans for a possible psychology book; with his encouragement I set to work on my first book, *An Introduction to Psychology,* published in October, 1901. It is a systematic treatment of experience from the double standpoint of atomistic and of self-psychology. I followed it up in 1905 by a summary of its teaching which I wrote in German and published (it is needless to add, after revision by a German friend) under the title *Der Doppelte Standpunkt in der Psychologie.* Atomistic and self-psychology figure in this treatise as *Vorgangspsychologie* and *Ichpsychologie,* fortuitous names, as Vaihinger was good enough to write me.

My psychological efforts, in the first years of the 1900's were largely directed toward replying to my critics. Their objections to my doctrine may be grouped roughly somewhat as follows. First, difficulties of detail, many of them justified—the objection, for example, that in perceiving one is not, as a fact, always conscious of other selves as sharing one's experience. Secondly, the criticism that in treating atomistic psychology as the only alternative to self-psychology I ignored the advancing claim of functional psychology. Thirdly, a charge of inconsistency with my own self-psychological doctrine. My definition of the idea, or mental process, as an experience taken without reference to self was (rightly, I think) claimed as tacit admission that the self is not essential to psychology. Fourthly, and most important, criticisms of my concept of the self as vague, unscientific, and unverified.

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[21] Cf. Münsterberg's paper on "Psychology and history" in the volume, *Psychology and Life,* 1899. For "history" he later substituted the term 'purposive psychology.' Cf. *Psychology, General and Applied,* 1915, chapters II and XI-XV. For my comment, cf. the paper, already cited, "Psychology as science of selves," and a review, *Psychol. Bull.,* 1914, **22**, 38 ff.

My immediate reaction to the second of these charges was embodied in an address, read in December, 1905, to the American Psychological Association. In this paper, "A Reconciliation between Structural and Functional Psychology," I interpreted the 'function' as fundamentally a reaction of conscious self on its environment and argued that "consciousness which always implies a conscious self is a complex alike of structural elements and of relations of self to environment."[22] With most of the other difficulties I dealt in a series of papers contributed in late 1907 and in January, 1908, to the *Journal of Philosophy*.[23] These constitute, once more, a sort of program for the second of my systematic treatises, *A First Book in Psychology*.[24] At many points of detail this book profits by the criticisms on its predecessors. It offers, not indeed a definition, but a description of the self as persistent, unique, complex, and also as related to objects, personal and impersonal. The book diverges most strikingly from those which preceded it by its abandonment of the duplex conception of psychology, as science alike of succeeding mental events and of the conscious self, in favor of a single-track self-psychology. In my preface, I call attention to the fact that I make the change "not because I doubt the validity" of psychology of the atomistic type but because "I question the significance and the adequacy, and deprecate the abstractness, of the science thus conceived." A second point of difference, due obviously to the influence of the functionalists and early behaviorists, consists in the emphasis laid on those "characteristic bodily reactions on environment which accompany perception, thought, emotion, and will." And, finally, an effort is made, in later editions of the book, to prune it of expressions tarred with the atomistic brush. In particular, the fourth edition formally abandons my earlier view, frankly acknowledging it as a "survival in my thinking of idea psychology," that the so-called structural elements of consciousness "are discovered only by an analysis of consciousness which leaves the self out of account." The second and systematic part of the present paper will treat in more detail the contents of this volume and will more carefully consider the criticisms urged against the personalistic psychology which it sets forth.

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[22] *Psychol. Rev.*, 1906, **13**, 76.

[23] Cf. **4**, pp. 673-683, and **5**, pp. 12-20, 64-68, 113-122.

[24] First edition, 1910; fourth and latest revised edition, 1914.

My psychological activities since the issue of this last edition of *A First Book in Psychology* have consisted in attempts to elucidate, to enrich, and to defend self-psychology. Even a recent paper on "The Ambiguous Concept of Meaning,"[25] seemingly immune by title from self-psychology, really takes its start in a criticism of the Titchenerian habit of dismissing the self as object of mere 'meaning.' With more express reference to the problems of self-psychology, I have carefully distinguished the psychologist's self from the philosopher's soul and have protested against the expulsion from psychology of the self along with the soul;[26] I have tried to show that the self is an object acknowledged or unacknowledged, of scientific observation and even of experimentally guided introspection;[27] I have argued also that the positive contributions of the so-called 'new' psychologies—behaviorism,[28] 'hormic' psychology,[29] Gestalt psychology,[30] and even the fundamental doctrines of the psychoanalysts[31] all fall naturally into place within the comprehensive system of a personalistic psychology. I have proposed accordingly as uniting concept for the warring systems the biological form of personalistic psychology, that is, the conception of psychology as science of the conscious organism.[32]

II

The preceding pages tell enough and more than enough about myself, my interests, and my occupations. In what follows I shall check my autobiographical outpouring and shall whole-heartedly devote myself, first, to setting forth and, secondly, to arguing for the essentials of a personalistic psychology. For with each year I live, with each book I read, with each observation I initiate or confirm, I am more deeply convinced that psychology should be conceived as the science of the self, or person, as related to its environment, physical and social. To establish this doctrine seems to me the first task of psychology and the essential preparation for its most important special undertakings.

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[25] *Amer. J. Psychol.,* Washburn Commemorative Volume, 1927, **39**, 7-22.

[26] "The case of self against soul." *Psychol. Rev.,* 1917, **24**, 78-300.

[27] Cf. "The self in scientific psychology." *Amer. J. Psychol,* 1915, **26**, 495-524. "Fact and inference in Wheeler's doctrine of will and self-activity." *Psychol. Rev.,* 1921, **28**, 356-373.

[28] "The truly psychological behaviorism." *Psychol. Rev.,* 1921, **28**, 1-18.

[29] "McDougall's treatment of experience." *Brit. J. Psychol.,* 1923, **13**, 337-343.

[30] "Critical comments on the *Gestalt-Theorie.*" *Psychol. Rev.,* 1926, **33**, 135-158.

[31] "The self-psychology of the psychoanalysts." *Proceedings and Papers of the Ninth International Congress of Psychology,* pp. 110 f.

[32] "Converging lines in contemporary psychology." *Brit. J. Psychol.* (*Gen. Sec.*), 1926, **16**, 171-179.

1) Self-psychology thus defined obviously is a form of introspectionist psychology. At the outset, therefore, I shall plainly state my reason for rejecting behaviorism, the one doctrine which, calling itself a psychology, none the less challenges the introspective procedure. By behaviorism I emphatically do not mean the doctrine set forth in the reiterated statements that consciousness is in its very nature impulsive, that any effective thinking must eventuate in doing, that we learn to think by learning to do. For all these commonplaces, popularly used in support of behaviorism, are perfectly consistent with introspective psychology and indeed form part and parcel of the output of all contemporary psychology, at least from William James down. They therefore constitute no argument for behaviorism proper, extreme behaviorism, the doctrine that so-called consciousness literally is, consists in, bodily reactions; that seeing is eye-movement; that emotion is chaotic instinctive reaction; and that thinking is internal speech. These statements, constituting as they do the center and core of behaviorism I oppose much as I should oppose the statement that a flame consists in striking a match and that the sound of a bell is an electric contact. Striking the match, as every one knows, is not identical with the flame: the two are related, in this case as condition and conditioned, but are not the same; and similarly the laryngeal muscle contraction, however closely related to thought, is not identical with thinking. In truth, if the two, thinking and sub-vocal muscle contraction, were identical, we should be wholly unable to explain the admitted expression of the same thought by phonetically dissimilar words. If, for example, the experience of 'equality' consists in the sub-vocal contraction of throat muscles involved in pronouncing the word, it cannot also consist in the quite different muscular contractions involved in "whispering to one's self" the word *Gleichheit.*

Accordingly, I reject behaviorism as a positive doctrine simply because, as has just appeared, it autocratically identifies phenomena which are to observation distinct. Behaviorism in its critical capacity I cannot, however, so summarily dismiss. The behaviorist as a critic calls attention to the difficulties of introspection—the fleetingness of experiences, the tendency of introspection to change its own object. But he chiefly protests against the "subjectivity," by which he means the individuality, of introspection. He stresses the fact that one can introspect only one's own private experience, that one cannot therefore check up or verify one's results—in a word, that the introspectionist must abandon the firm ground of natural science. Now no introspectionist will deny the difficulty or the fallibility of introspection. But he will stoutly urge against the behaviorist, first, that this argument is a boomerang telling against "the firmly grounded natural sciences" as well as against psychology. For the physical sciences themselves are based in the end on the introspections of scientists—in other words, the physical sciences, far from being wholly free of 'subjectivity' must describe their phenomena in the sometimes diverse terms of what different observers see, hear, and touch. In the second place, as the discriminating critic of behaviorism points out, the introspective psychologist does not actually confine himself to the study of his own private experiences, though he certainly starts from them. Rather, he attributes to his fellows experiences resembling his own, indicated to him by their speech or by their non-verbal behavior. In a word, the introspective psychologist deals not only with his own directly introspected experiences but with the inferred experiences supposedly introspected by other people. For both these reasons I refuse, at the behest of the behaviorist, to abjure the study of the mental life. But this, as a later section will set forth, means only that I refuse adherence to the negative part of behaviorism, its denial of self and of consciousness. On the other hand, like all introspectionists, I welcome cordially every positive contribution of behaviorism—every study of conditioned reflex and bodily response. For it is an admitted part of the psychologist's business to correlate bodily reactions with conscious experiences—immediate reactions, for example, with perceiving, delayed reactions with deliberation, chaotic and interrupted reactions with emotion. Introspectionists of varying types may conceive the correlation differently, but all assert it.

2) The conclusion thus achieved that psychology is essentially introspective, falls far short, however, of defining my position. For the term 'introspective psychology' shelters two widely different types of psychological system—the impersonalistic and the personalistic. Under the first head are included systems of widely different char-

acter which, however, are alike in one respect: whether they conceive psychology as concerned with ideas, states, or contents of consciousness, mental processes, or experiences, with functions, with urges or drives, with complexes or with *Gestalten,* they ignore or deny the self, the person or organism which is conscious, which experiences, which functions, which drives or is driven. Personalistic psychologies, on the other hand, conceive their science as consisting basally in the study of conscious, experiencing, functioning beings, that is, of persons or selves. I have already avowed my adherence to this personalistic doctrine. I base my conviction simply and fundamentally on my direct experience, my observation—corroborated, as it is, by that of other people. Whenever, in truth, I try to take the opposite point of view, when, in other words, I attempt the study of mental processes, experiences, and the like, I invariably find not a mere process, an experience, but a mind in process, a someone who is experiencing. In a word: I am a personalistic, introspective psychologist because in introspection I find the self.

But I have not even yet adequately delimited my conception of psychology. For personalistic psychology also is of two main types: first, the strictly psychological, to which is applied sometimes the term self-psychology in a narrow sense of the phrase, and, secondly, the biological. This biological form of personalistic psychology studies the psychophysical, or better the psychosomatic organism, mind in body, or conscious organism, and conceives consciousness as one response among others, though a peculiarly important response, of organism to environment. The first and more strictly psychological form of personalistic psychology, though it does not disregard the neurological correlates of experience, the muscular reactions which accompany different mental attitudes, and the biological values of consciousness, none the less teaches that the self has a body and is not, in any sense, constituted by body. The neurological, physiological, and biological data serve from its point of view to classify and in a sense to explain mental phenomena.[33] To this narrower type of self-psychology I subscribe, largely for the reason that it seems to me required by the distinctions actually made by the biological personalists themselves between the "merely physiological" and the "purely psychical" bodily reactions—between assimilation, for example, and sensibility.[34] I shall devote the next following pages to a brief exposition of self-psychology, thus conceived.

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[33] Cf. my "Is the self body or has it body?" *J. Phil.*, 1908, **5**, 12-20.

a) Self-psychology has three basal conceptions: that of the self, that of the object, and that of the self's relation or attitude toward its object. In the concrete terms of Knight Dunlap, "when I look at the page in front of me there are three aspects of the situation involved: the I, the page which I see, and the fact of seeing the page."[35]

(1) By self, or I, is meant what every one of us means by such expressions as "I am ashamed of myself," "I approve of myself," "I appeal to you, yourself." Like 'consciousness,' the 'self' is, strictly speaking, indefinable—for it is *sui generis* and cannot be assigned to any including class save that of 'the existent.' The self, however, though indefinable, is describable; its characters, in Miss Gamble's phrase, are 'properties,' not 'differentiae.' These characters, whether silently assumed or explicitedly stated, include at least the following. The self is, first of all, (*a*) a totality, a one of many characters and of many temporal signs; is, secondly, (*b*) a unique being in the sense that I am I and you are you—that no one, however similar, can take the place of you or of me; is, thirdly, (*c*) an identical being (I the adult self and my ten-year-old self are in a real sense the same self); and yet is also (*d*) a changing being (I the adult self differ from that ten-year-old). Finally, (*e*) the self is a being related in a distinctive fashion both to itself and its experiences and to environing objects, personal and impersonal. This relation to all these objects is called its consciousness of them.

No one, of course, is attentively aware of all these characters of the self—of totality, identity, change, uniqueness, and of relatedness, or consciousness—as distinguished from each other, on all occasions when one "observes one's self," any more than one is distinctly aware of sensitivity, motility, assimilation, and reproductivity whenever one observes an animal. Yet in my opinion it is true not only that I from time to time directly observe myself as characterized in each of these ways but that I may have also a direct, if fused, awareness of myself as possessed of all of them.

(2) It has just appeared that the self is conscious of objects. In this way, the object makes way into psychology in spite of the protests of the writers who, while constantly referring to objects,

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[34] Angell, J. R. "The province of functional psychology." *Psychol. Rev.*, 1907, 14, p. 82 and Note.

[35] *Elements of Scientific Psychology*, pp. 22-24.

none the less officially bar them from psychology. The term is used in the wide sense suggested by McDougall when he says that "experiencing is an activity of some. . . .subject who experiences something or somewhat."[36] This somewhat-which-is-experienced, whatever its nature, is the object. It is sharply to be distinguished from the stimulus, physical or physiological (ether-waves or retinal excitation, for example), of which the experiencing self is seldom directly conscious. Objects of the self are marked off from each other in several ways: as either personal or impersonal, and if impersonal as either physical or logical; and as either private or public. My private objects, all of them personal, are myself and my experiencings; but my public objects, those which are your objects as truly as they are mine, are either personal or impersonal. To illustrate: (*a*) my interest in Lord Haldane's autobiography is my private, personal object; (*b*) Lord Haldane himself is my public, personal (or social) object; (*c*) the cover of the bound volume is a public, impersonal, physical object; (*d*) the Hegelian philosophy set forth by Lord Haldane is likewise a public and impersonal, but a logical not a physical object. (My own body occupies a curious midway position between the group of public and that of private objects: it is the object of your visual and pressure consciousness as of mine, yet I do not share my kinaesthetic and pain sensations with you.)

(3) Towards its objects, thus distinguished, the self-psychologist conceives the self as taking certain basal attitudes. These attitudes, or fundamental relations of self to its objects, seem to me to fall roughly into several groups.[37] To the first of these groups belong receptivity, activity, and what I can only call the feeling of being compelled. (*a*) I am always receptively conscious and my receptiveness is of different sorts. For example, I receptively experience not only the fleecy whiteness of the clouds but also their charm (or pleasantness) and the contrast between the blue of the sky and the whiteness of the clouds. In other words, I am sensationally, affectively, and relationally receptive. Besides being receptive, as apparently I invariably am, I am often (*b*) active. My activity takes

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[36] *An Outline of Psychology*, p. 221¹. Cf. p. 40.

[37] For the account which follows, I am responsible, though for the most part in accord with my colleague, Professor Eleanor Gamble. But though the specific analyses and much of the terminology are my own, I find that personalistic psychologists agree with me not only in essentials but, implicitly if not explicitly, in many details. Cf. my "Converging lines in contemporary psychology." *Brit. J. Psychol.*, 1926, 175 f.

one of two forms: either that of wishing, longing, yearning, or else that of volition. Everyone recognizes a difference between wishing and willing. Each is a form of self-activity, sharply contrasted with the receptivity of perception and of emotion, but wishing is an unassertive and willing is an assertive form of activity. Will, assertive self-activity, is of two main types, that is to say, I assert myself in one of two ways, either (*a*) by dominating somebody or something, the hostile audience whom I am addressing or the intractable sailcloth which I am stitching, or else (*b*) by active adoption of another's cause, by active loyalty to friend or to leader.[38] Finally (*c*) I sometimes feel myself compelled either by impersonal objects or by people. I may, for example, have this feeling of being compelled, in other words, I may experience my own impotence, in relation both to the wind, as it sweeps across Boston Common and to the imperious gesture of the traffic policeman.[39]

The basal personal attitudes of my second group are the egocentric and the allocentric. These distinguish the I, or self, as it stresses either itself or its environing objects. The egocentric emphasis may fall on one (or all) the characters of self: in recognition, for example, one is predominantly aware of self-identity, in emotion, of individuality. The allocentric attitude may have as objects either things, as in perceiving and imagining, or other selves, as in hatred or in reverence. The one attitude does not exclude the other—in other words, one may, at one and the same time, attend both to one's self and to one's object; and of this complex attitude sympathy is an especiall yimportant instance. For this is the awareness of one's self as sharing in the experience of other selves—an experience most characteristic of the life of emotion, though appearing occasionally in perceiving and in thinking.

The self finally either individualizes its objects as in emotion and will and, secondarily, in perceiving, imagining, and some forms of thinking; or it generalizes as in classification and conception.

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[38] Cf. my *A First Book in Psychology,* Chapters XII and XIII, especially pp. 226 f., 244 f.

[39] The preceding paragraph embodies several additions to my earlier teaching. The first of these is the treatment of the so-called structural elements, sensational, affective, and relational experience, as forms of receptivity. The second is the broadening of the conception of activity to include wishing. The recognition of the feeling of being compelled as distinct alike from mere receptivity and from assertiveness I also for the first time propose. It is virtually the distinction made, years ago, by Michotte. (Cf. *Arch. d. Psychol.,* 1911, **10**, 195[3].) The experience is constantly reported by introspecters in an as yet unpublished study of choice, made at Wellesley College.

All these distinctions are brought together in the following summary.

Basal Attitudes of the Self

1) The self is
   a) always receptive
      (1) always sensationally receptive
      (2) sometimes affectively receptive
      (3) usually relationally receptive
   b) often active, i.e.,
      (1) often wishes (is unassertively active)
      (2) sometimes wills (is assertively active)
         (*a*) imperiously
         (*b*) adoptively
   c) sometimes conscious of being compelled
      (1) by people
      (2) by things
2) The self is always
   a) egocentric, or
   b) allocentric, or
   c) both egocentric and allocentric (and sometimes sympathetic)
3) The self
   a) sometimes generalizes
   b) sometimes individualizes

In their bald enumeration these lists of characters and attitudes of the self and of the types of its objects may seem to the full as non-essential and as dull as the Homeric catalogue of ships or the roll of "gentlemen with very hard names" in the Books of the Chronicles. Yet I believe that anyone who, without bias, will study the material of psychology by the use of these categories will discover them for what they are—not impositions on experience but descriptions of it. To supplement the illustrations already given—perceiving, imagining, and thinking, the chief forms of the cognitive consciousness are marked off from the non-cognitive experiences, emotion, volition and the like, by the allocentric attitude of cognizing selves, that is, by inattention to themselves, and absorption in their objects. In the non-cognitive experiences, on the other hand, every one is highly egocentric, is poignantly aware of himself as a unique self either grieving and joying, loving and hating as never self grieved and joyed and loved and hated or else as actively asserting himself in dominating or in loyal attitude. In the social form of emotion and of will, in compassion, for example, and in cooperation, a man may also, it is true, stress not only himself but other selves as well; but such experiences are never wholly allocentric—in neither of them can one lose the "vivid sense of one's self" in which consists what we have called the egocentric attitude.

In the preceding paragraph I have tried to show how the non-cognitive experiences, as a whole, are marked off from the cognitive by use of the distinction between the allocentric and the egocentric attitude of the self. For the differentiation, within the group, of emotion from volition one must turn to the distinction between assertiveness and the feeling of being compelled. Both emotion and will are, as has appeared, essentially egocentric experiences but in emotion I as passive, prostrate, the victim of my environment, carried high on the crest of the wave of prosperity, or sucked into the whirlpool of disaster, whereas alike in will and in loyalty, that is, in imperious and in adoptive self-assertion, I am the maker of my own way, the "master of my fate."[40]

b) In the preceding pages I have boldly summarized the basal features of that type of personalistic psychology which, along with Ward, Mitchell, Rehmke, Gamble, and others, I profess. But I am quite as anxious to set forth the closely allied conception of the biological personalists in psychology, of William McDougall, Stern, Stout, Angell, and others. These writers, in the first place, unequivocally oppose every form of impersonalistic psychology. "The psychic datum (*das Gegebene*)," says Stern, "must be given to some one."[41] "One might as well," McDougall declares, "expect to find a 'falling' or a 'movement' without something that falls or moves as 'a perceiving' or 'a remembering' detached or isolated from the subject who perceives or remembers."[42] *"Nicht es empfindet sondern ich empfinde,"* Müller-Freienfels asserts.[43] Thus, like the strict self-psychologists, these writers treat psychology as science of the person, or self. In their view, however, as I have already indicated, this basal unit of psychology, the person, I, or experiencer, is not a purely mental self, but is rather the 'embodied self,'[44] the conscious organism. In other words, they conceive the self, or person, as the organism in response to its environment,[45] and among its responses they include not only consciousness but also biological adaptation, nerve-excitation, muscular contraction, secretion, and nutrition, though they stress consciousness as a distinctive and supremely significant response. I have already stated unequivocally my own choice of the strictly psychological conception of the self-which-has-a-body in preference to this doctrine of the embodied self.[46] I am convinced, none the less, that this biological form of personalistic psychology provides a middle ground in which most schools of contemporary psychology may meet. Hence I am anxious to emphasize the close affiliation of the two forms of personalism. To consider in the first place what I have called the characters of self: these psychophysical personalists, one and all, stress (1) the totality (*Ganzheitlichkeit*) or, as Stern calls it, the *unitas multiplex* of the self.[47] "In every single sensation," Müller-Freienfels asserts, "in every single act of will, the whole I acts together, and only from the standpoint of this totality of the I can the so-called constitutive elements be understood."[48] Implicitly, and often explicitly also, personalists of this group, in the second place, conceive the self as unique, or individual. Müller-Freienfels suggests the uniqueness in his doctrine of the self as opposing others (*gegenstellend*). Stern brings together both the totality and the individuality in his definition of the person as "such an existent as, spite of the plurality of its parts, exhibits a real and distinctive . . . unity;"[49] and he refers in another passage to a "last unique quality (*ein letztes Ureigenstes*) by which every person is contrasted with every other."[50] In the same context Stern implies the identity in change of the self; and, finally, throughout his psychological writings, like all these biological personalists, he conceives and treats the self as variously related to environing objects of different types.[51]

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[40] Cf. for detail the relevant chapters of my *A First Book in Psychology.*

[41] *Die Psychologie und der Personalismus,* p. 15^4.

[42] *Outline of Psychology,* p. 40^2.

[43] *Das Gefühls- und Willensleben,* p. 15^1.

[44] Cf. Stout, in "Mind, objectivity, and fact," *Aristotelian Society.* Supplementary Vol. VII, 1927, The nature of introspection, p. 85^2.

[45] Cf. Stern, *Die Psychologie und der Personalismus,* pp. 42^5, 35^4.

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[46] For a more detailed discussion of this issue, cf. *J. Phil.*, 1908, **5**, 13 f.

I may refer also to my criticism of the doctrine common to most if not all of these psychosomatic personalists, that the self is always, consciously or unconsciously, purposive. In opposition to this, I have pointed out, first, that unconscious purpose is a contradiction in terms and, secondly, that the self though often, is not invariably, purposive. (For statement of this doctrine, cf. Stern, *Die Menschliche Persönlichkeit*, pp. 19, 134, 142; McDougall, W. *Outline of Psychology*, pp. 47, 53 and *passim*. For my criticism, cf. my "Converging lines of contemporary psychology," *op. cit.*, pp. 176 f., with footnote 3, p. 177.

[47] *Die Psychologie und der Personalismus*, pp. 6^3 *et al.*

[48] "In jeder einzelnen Empfindung, in jeder einzelnen Willenshandlung wirkt das ganze Ich mit, und nur von dieser Ganzheit des Ich aus sind die augeblichen konstitutive Elemente zu verstehen." *Op. cit.*, p. 18^2.

[49] *Die Psychologie und der Personalismus*, pp. 7^2, 42^2.

[50] *Die Menschliche Persönlichkeit*, p. 95.

It follows, of course, that personalistic psychology of the biological type is, as obviously as strict self-psychology, concerned with objects. And in curiously close resemblance to my own classification of these objects, though in entire independence of it, Stern distinguishes them as *Überpersonen* (people, races, and the like), *Nebenpersonen* (our fellowmen) and *die Ausserpersönliche* (impersonal objects).[52]

In the discussion, finally, of personal attitudes, or types of response, the two schools of personalistic psychology are closely alike. This is especially evident in the writing of Müller-Freienfels and Stern, most systematic of these psychosomatic personalists. Both suggest the conception of volition as active; both treat with special emphasis the contrast between the allocentric and the egocentric (in their own terminology, between the 'objective' and the 'subjective') attitude. The 'objective' attitude, as each writer expressly declares, distinguishes the life of perception and imagination and thought while the 'subjective' attitude characterizes emotion and will.[53] Stern indeed classifies his dispositions and his basal purposes primarily as egocentric and heterocentric. And Müller-Freienfels, in even completer agreement with self-psychology, expressly includes also the participatory attitude, *Einfühlung,* or *Wirbewusstsein* as he often felicitously calls it, contrasting it with a *Gegenfühlung* in which I find a suggestion of what I have called the awareness of one's own individuality.[54]

It should be added that, just as these categories of a strict self-psychology are used by the psychosomatic psychologists, so nothing forbids the self-psychologists from enriching their doctrine by distinctions stressed by these biological personalists. So, for example, the psychological as well as the biological self may perfectly well be credited with dispositions, that is, with "chronic tendencies and attitudes of the person toward the achievement of definite ends"; and the psychologically as well as the biologically basal egocentric attitudes may be distinguished as involving either self-preservation or self-development. In brief: personalistic psychologists of both types, the strictly psychological and the biological or psychosomatic, agree firmly on their conception of psychology as science of a conscious being, a one of many characters, individual, self-identical, and changing, in varying reaction on an environment personal and impersonal. And they describe, in essentially similar terms, the nature of these reactions or responses.

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[51] Cf. Stern, *Die Menschliche Persönlichkeit,* Chap. III, p. 95; McDougall W., *Outline of Psychology,* Chapters II-IV.

[52] *Die Menschliche Persönlichkeit,* p. 115.

[53] Müller-Freienfels, *Das Gefühls- und Willensleben,* pp. 239[4], 42; Stern, *Die menschliche Persönlichkeit,* pp. 19, 23 f.

[54] *Op. cit.,* pp. 151 ff.

3) It should be evident that personalistic psychology, in either of its forms, is entirely compatible with the significant positive content of every other system of psychology, and that accordingly one may become a personalistic psychologist without giving over any positive doctrine whatsoever. I shall, none the less, devote a few paragraphs to the elaboration of this statement.

a) To begin with so-called structural, or existential, or idea-psychology—its basal features are, as everybody knows, the following: it deals with specific movements of experience, cut off from an experiencer; it analyzes these experiences into sensational, affective (and perhaps relational) elements. In practice, also, structural psychologists use, wherever possible, an experimental method; and are disposed to seek what they call explanation of psychic events in physiological phenomena, observed or inferred. But personalistic psychology has a place for all these doctrines. To take them up in reverse order: the self-psychologist, as well as the idea-psychologist, may correlate psychical with physiological data. It is as easy, for example, to correlate the disintegration of a retinal substance and occipital lobe excitation with a self's visual perceiving as with a visual percept. The self-psychologist may furthermore introspect under experimental and "standard conditions," as a later section of this paper will show in more detail. As regards analysis, self-psychology from the first has recognized the so-called structural elements, insisting that it is quite as correct to say that the self is sensationally and affectively conscious as to say that an experience is made up of such and such sensational and affective elements. Even the study of experiences, rather than experiencer, may be tolerated by the self-psychologist, provided it is carried on openly in avowed abstraction from the admittedly existing self who experiences. Only the great negation of existential psychology, its outlawry of the self, its insistence on contents or ideas or experiences as the one concern of scientific psychology, is inconsistent with personalistic theory.

b) With the *Gestalttheorie* self-psychology is essentially in agreement. Both, in the first place, stand out determinedly against all forms of atomistic doctrine. And personalistic psychology, in the second place, perfectly accords with the conception alike of experiences and of physical objects as wholes of subordinate parts and not mechanically added sums of external units. Finally, the self or person—though most Gestaltists have notoriously overlooked the fact[55]—is the supreme illustration of the *Gestalt*—an integrated, complex whole inclusive of parts and characters subordinate to its own distinctive unity.

c) This suggests the significant resemblance of self-psychology to behaviorism: each treats primarily of entities, organic wholes, and not of abstracted states or processes. But the likeness goes further. With behaviorism and its forerunner, functional psychology, personalistic psychology, in both its forms, shares the significant conception of relation or attitude toward environment. Activity and passivity, allocentric attention and sympathy—all these unquestionably are forms of response to environment.

d) Self-psychology is finally at the core of every one of the psychoanalytic systems. Not only does the conscious ego play a rôle, if only a minor rôle, on the psychoanalytic stage, but even the unconscious closely studied turns out to resemble nothing so much as a dissociated self. Characteristic conceptions of the psychoanalysts prove the same point. Neither the censor, for example, of Freud's earlier books nor the super-ego of his later period can be impersonally conceived; Jung's distinction between extroversion and introversion, as positive and negative relation between subject and object, presupposes the existence of self and of object; Adler's emphasized contrast between the sense of power and the feeling of inferiority clearly requires the experience of one's self in relation to other selves.

4) From the vantage ground of my brief sketch of self-psychology, I propose next to consider briefly the more important of the objections urged against it, passing over entirely the captions, the merely verbal, as well as the minor criticisms.

a) The first of these significant difficulties, vigorously stressed by Titchener,[56] is that the self, though an object of uncritical, everyday awareness, is no proper object for the scientist's technical consideration. This objection, however, makes the unwarranted assumption of one class of objects for the plain man and quite another for the scientist. In opposition to this view, and in agreement with the vast majority of scientists, the self-psychologist maintains that science differs from everyday experience not in its objects, but in the method, analytic, classificatory, and explanatory, in which it treats the objects which the plain man uncritically "swallows whole." To state this differently: in the view of the self-psychologist, the scientist observes what the plain man observes—acids, steam, flashes of light, birds, rocks, stars, and selves—but observes all these analytically, and is at pains to group and to link, to classify, and to explain the objects of his observation. As self-psychologist, accordingly, I not only admit but insist that the self is an object of everyday consciousness. I, however, flatly deny that this prevents the self from being also an object of the psychologist's study. And I point to the distinctions, which preceding pages present, of the characters, objects, and attitudes of the self as indications of the type of analysis characteristic of self-psychology.

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[55] The exception is Koffka. For his doctrine of the conscious organism cf. his paper on "Introspection." *Brit. J. Psychol.*, 1924, **15**, 153.

[56] "Description vs. statement of meaning." *Amer. J. Psychol.*, 1912, **23**, 167.

b) A more common criticism relegates the self to metaphysics, or perhaps to ethics, as opposed to science. This objection, urged from the very outset, is constantly reiterated. To quote Professor J. S. Moore's statement of it: "To speak of the self as anything more than a sum-total of phenomena is to leave the bounds of science and enter the realm of metaphysics."[57] It is easy to account for this criticism. In its wholly justified attempt to avoid entangling alliance with philosophy, modern psychology has quite correctly rid itself of the metaphysician's self—the self often inferred to be free, responsible, and immoral[58]—and has thereupon naïvely supposed that it has thus cut itself off from the self. But the self of psychology has no one of these inferred characters: it is the self, immediately experienced, directly realized, in recognition, in sympathy, in vanity, in assertiveness, and indeed in all experiencing. The psychological concept of self forms, to be sure, the core of the metaphysical self-doctrine, but the two are not identical.[59] For the self is, in the first instance, not an inferred reality but an observed fact.

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[57] *The Foundations of Psychology*, p. 15^2.

[58] I am not denying the validity of these inferences, but am merely concerned to brand them as non-psychological.

[59] To claim as Roback does that "to speak of a permanent self is to commit oneself to a purely idealistic conception of psychology" is to ignore the fact that personalistic psychology is compatible with any save a genuinely materialistic metaphysics. In confirmation of this conclusion I may appeal to John Laird, unequivocal realist, who none the less says that "desiring, willing, and knowing do not float around loosely. They always unite in a personality" (cf. "A study in realism," p. 173).

All this is merely a restatement of the rejoinder which for years I have been making to this railing accusation that the self is a metaphysical concept. I want explicitly to supplement it by the reminder that the criticism, whatever its force, applies only to the self in its narrower sense and not at all to the self as psychosomatic personalists conceive it. For assuredly the living, breathing, secreting, reacting body—even if also a conscious body—may be accepted as a proper object of scientific study.

c) The most menacing of all the criticisms of personalistic psychology has, however, still to be stated, and will, if justified, completely undermine its foundation. This is the sheer denial that the self really is observed directly.

d) And a final objection urges that the self, even if admitted to psychology, would make little difference: "The barren reassertion," Robert MacDougall says, "that in each fact is the self adds nothing to its treatment."[60] I propose to discuss both objections in the following section of this paper, for I base my support of personalistic psychology squarely upon the exact contrary of each of these assertions.

5) The issue is clearly drawn. Personalistic psychologists, and in particular self-psychologists, deliberately argue for their doctrine on precisely the grounds upon which their critics reject it. Impersonalistic psychologists deny, in the first place, and personalistic psychologists claim, that the self is directly observed. The impersonalist argues thus: if the self were immediately experienced it would be universally experienced, whereas few psychologists, and few or no experimental psychologists, working under standard conditions, either affirm or admit the existence of a self.

a) The personalistic psychologist takes the following position to the charge that the self is not an object of scientific observation.

(1) He of course admits that many introspective reports make no mention of a self. But he very readily explains the omission. The awareness of self is by its very nature a constant experience, likely therefore to be inattentively observed and neglected in report, somewhat as introspectors forget to report the constant pressure of the atmosphere. Introspectors, in the second place, are seldom taught to look for the self—they may even be told expressly that the self is not an object of introspection. Finally, the great body of experi-

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[60] *The General Problems of Psychology,* p. 186^2

mental investigation is still concerned primarily with perceptual experiences and secondarily with discrimination, comparison, and other sorts of thinking. Not unnaturally, reports of introspection in these cases include little or no reference to self, since precisely in perceiving and in thinking we are very inattentively aware of ourselves.

(2) The self-psychologist, however, does not for a moment admit that all experimentally controlled introspections lack reference to self. He points, on the contrary, to three groups of experimental investigators of the nature of choice,[61] working in widely separated laboratories and employing wholly different methods, who report and emphasize the experience of self. I shall devote the next following pages to an indication of these crucially important findings. For the objective results—reaction-times, respiration records, galvanometric deflections, and the like—I must refer to the detailed reports of the experimenters. Ach was specifically concerned to estimate the concentration of volition required to overcome associative habits. His subjects first learned series of *paired* nonsense syllables and were later required to respond in a novel fashion to each of the odd-numbered syllables in these learned series. If, for instance, the subject had learned a series of *rhymed* pairs of syllables, *zup-tup, mar-par, bis-zis, tel-mel,* he was required to respond successively to *zup, mar, bis,* and *tel* not by rhyming but by reversed syllables (*puz, ram, sib, let*). In the experience involved in this experimental procedure Ach's subjects distinguished four factors: first, the "perceptual phase, constituted by kinaesthetic sensations; secondly, the objective phase, the normally imaginal consciousness of the outcome of the volition; thirdly, and most significant, activity (*Betätigung*) the attitude (*Stellungnahme*), "I will"; fourthly and finally, the consciousness of exertion. Strictly speaking, in Ach's opinion, will consists in the third phase, activity, in which, Ach plainly states, the I is experienced (*erlebt*) not inferred.[62]

Experimentally controlled study of volition of a quite different sort, was initiated by Michotte and Prüm in the Louvain Laboratory and has been followed up by a series of experimenters, Barrett, Aveling, and Wells, in Louvain and in London. In these experiments the subjects chose "for a serious reason" between different procedures and then introspectively examined the period, the fore period and the after period of their experience. Michotte's subjects were shown a card containing two numerals and had to choose whether to multiply or to divide them; Barrett's observers were to reach out each for one of two odors; those of Wells for one of two tastes already familiar. All reported the occurrence, during the experience preceding their reaction, not merely of a wealth of sensation, predominantly kinaesthetic, but also of what they once more describe as consciousness of self-activity.[63]

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[61] The term choice is used in the sense of experience antecedent to a reaction to one of several alternative objects.

[62] On all this cf. Ach, N. *Über den Willensakt und das Temperament,* Chap. III.

From the Columbia University Laboratory comes an experimental study of a third sort which finds in choice an experience of self-activity. The investigator, Dr. Alfred Martin, used a method totally different from either that of Ach or that of the Michotte group. He directed each of his subjects to imagine himself in a certain dilemma and then to make a choice between two solutions. For example: "You are to attend a social gathering at a home not previously visited . . . Would you prefer . . . to go in evening dress with a chance of being made conspicuous or in ordinary dress and perhaps feel out of place?" All Martin's observers report as the final phase in their decision what he calls self-assertion which, he finds, invariably involves a self reference: the determination, "This is what I, *myself*, will really do."[64]

The personalistic psychologist finds support for his position even in the two experimental investigations, those of Wheeler[65] and of Amen,[66] whose authors expressly deny that their introspectors report the consciousness of self. My reasons for this high-handed challenge of the interpretations made by Wheeler and by Amen of the introspective records of their own observers are briefly these:[67] Both sets of introspectors assert the existence of self. "It was 'I,'" says one of them, "who did the figuring and regarded the answers and felt the effort of strain in attempting to get them correct."[68] "It was pretty definitely I," another says, "who was experiencing the sinking feeling. The sinking feeling wasn't just going on, it was my sinking feeling."[69] "A complex kinaesthetic and visual *schema*," one of Wheeler's subjects declares, "represented to me that I was in the act of 'accepting' this title as my choice."[70] Wheeler and Amen attempt to explain away these seemingly unambiguous examples of a consciousness of self, first, by the curious and entirely unjustified assumption that a consciousness of self, if it ever occurred, would be elemental;[71] and secondly, by the attempt to reduce this experience of self to impersonal terms, in Wheeler's case to sensations chiefly kinaesthetic, in Amen's case to a meaning-sensory-imaginal complex of the perceptual order. Both attempts are unsuccessful—Wheeler's because he leaves unanalyzed two technical terms, acceptance and self-imposition of instruction, both obviously involving the experience of contrasting selves; Amen's because her undefined term meaning conceals an implicit reference to consciousness of self.

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[63] Cf. Michotte, A., & Prüm, E. Étude expérimentale sur la choix voluntaire et ses antecedents immediats." *Arch. d. Psychol.*, 1911, **10**, pp. 113 ff. Barrett, E. B. *Motive force and motivation tracks*, 1911. Wells, H. M. "The phenomenology of acts of choice." *Brit. J. Psychol. Monog.*, 1927; No. 11. (It should be noted that Dr. Wells expressly recognizes the consciousness of self in experiences other than choice.)

[64] "An experimental study of the factors and types of voluntary choice." *Arch. Psychol.*, 1922, No. 51, p. 58.

[65] "An experimental investigation of the process of choosing." *Univ. Ore. Publ.*, 1920, **1**.

[66] "An experimental study of the self in psychology." *Psychol. Rev., Monog. Suppl.*, 1926, No. 165.

[67] For more adequate consideration of these investigations, cf. my "Fact and inference in Raymond Wheeler's doctrine of will and self-activity." *Psychol. Rev.*, 1921, **28**, 356-373, and my "Self awareness and meaning." *Amer. J. Psychol.*, 1927, **38**, 441-448.

b) In the face of these considerations candid critics of personalistic psychology must certainly abandon the charge that serious experimental introspections include no observations of the self. They will, however, recur to their more general position. If, they repeat, the self is, as the personalists claim, immediately experienced, then it should be observed and reported by everybody and this notoriously is not the case. (1) To this, as self-psychologist, I make the following reply: It is, of course, impossible categorically to deny the outcome of anybody's introspection. I cannot accordingly directly dispute the statement of the psychologist who asserts that he never finds a self. I can, however, convict him of naïve inconsistency in his emphatic assertion, I find no self. For who, I ask, is this I which denies that it observes an I?[72] In a word, I accuse my critic of assuming, in almost every paragraph, the existence of the very self whom he disbars. (*a*) In reply, the objector, if he runs true to form, will insist that his use of the pronoun 'I' is a mere language habit. By the phrases, "I remember," "I accept," "I sympathize," so he says, he means simply that "a memory," "an acceptance," "a sympathy," occurs. In a word, he is merely adopting the personalistic convention of language. And he contends that I have no more right to attribute to him a latent self-psychology than I have a right to foist on any one who "sees the sun rise" a Ptolemaic conception of the physical universe. (*b*) My critic cannot, however, hope by this facile retort to win for himself the privilege of hunting with impersonalistic hounds while he runs with personalistic hares. For the truth is that critics of self-psychology do not confine themselves to the casual use of expressions such as "I perceive," "I attend," "I feel." Rather, they employ the technical distinctions of the self-psychologist in analyzing and classifying psychological phenomena. To state this more definitely: the unequivocal opponents of self-psychology habitually define or describe psychological phenomena not merely in terms of sensations, complexes, patterns, and what-not, but in terms also of the conscious self. The pages which follow abundantly substantiate this statement and I know no treatise on psychology which does not illustrate it. My initial argument for self-psychology is, accordingly, simply this: that even its opponents persistently invoke the self in systematic exposition and description, whereas it is contrary to all canons of science at once to employ and to outlaw a given conception. Either all references to any self should be eschewed or the self should be given a standing in psychology.

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[68] Amen, *op. cit.*, p. 49 (Observer M).

[69] Amen, *op. cit.*, p. 48 (Observer F).

[70] Wheeler, *op. cit.*, p. 12[3] (Observer J).

[71] Cf. Wheeler, *op. cit.*, pp. 29, 51; Amen, *op. cit.*, p. 72.

[72] Cf. Gamble, E. A. McC. "A defense of psychology as science of selves." *Psychol. Bull.*, 1915, **12**, 196.

(3) The serious critic of self-psychology, if I understand him rightly, proposes to accept this conclusion. Indeed, he urges, as has already appeared, that the self, if admitted to psychology, would be of small value or, in the words of one of these critics, make "very little return."[73] This rejoinder leads directly to my final argument for the self in psychology. I have just urged that critics of self-psychology constantly describe psychic phenomena in personalistic terms. I claim now that they inevitably use these terms. To state this differently: I assert unhesitatingly that there are certain experiences, admitted by every introspectionist and by most behaviorists to be subject-matter of psychology, which simply cannot adequately be described save in terms of the characters and attitudes of the self; that the self is consequently neither an avoidable nor an empty concept in psychology. Recognition is a classic instance. Everybody is

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[73] Roback, A. *Behaviorism and Psychology,* 1923, p. 264.

familiar with J. S. Mill's annotation on the associationist teaching of James Mill. "Memory," he says, "is having (an) idea recalled along with the belief that the fact, which it is idea of, really happened....and....to myself....who formerly experienced the facts remembered, and who was the same ego then as now."[74] Less familar is a similar statement by Titchener, uncompromising critic of self-psychology. Arguing against the notions of a memory-idea as copy of past experience, he says: "A verbal-motor image....may mean for A some visual object that he perceived so many years since."[75] The phrase "for A" is, of course, no more nor less than a thinly veiled reference to a self.

Other examples of these experiences which must be described in terms of self-psychology are sympathy, vanity, and trust. It is plainly impossible to distinguish sympathetic from unsympathetic joy or grief by enumeration of organic sensations and affective elements, for these are or may be precisely similar in the two cases. What, for example, distinguishes my sympathy in your loss of a fifty-dollar bill and my regret at my loss, while in your company, of a similar bill? From the impersonalistic point of view, there occur in each case, first, visual images of a bill, a purse, your figure, and places in which the loss might have occurred; secondly, visceral sensations, diaphragm pressure and the like; thirdly, sensations due to changes in the circulatory and vascular systems; fourthly, affective unpleasantness. But these factors are not significantly different in the two cases: in the end, one has to distinguish the two experiences on the ground that in one and not in the other of them I feel myself to be sharing the consciousness of someone else. In similar fashion, one falls short of the distinction between trust and imperiousness if one fails to contrast the self-subordinating with the dominating attitude; and one slurs the difference between vanity and pride if one ignores the reference in vanity, and not in pride, to the shared estimate of one's self by admiring fellow-selves.

The psychology of the social situation teems with similar instances. Instructive examples are found in the efforts of impersonalistic psychologists to deal with the relation of observer to experimenter. Titchener, for instance, quotes an observer's report: "act of acceptance of essentially kinaesthetic character felt as belonging to the self-side of experience," and interprets "the latter phrase" as meaning that "the reactor felt himself in the attitude of acceptance, irrespectively of the actual physical attitude of the body."[76] Such an attitude of acceptance, expressly contrasted with a bodily attitude, is of course personal. Imitation and initiation, leadership and docility, fundamental categories of social psychology, offer other examples of experiences meaningless unless conceived as relations of selves to each other. And by selves, I may venture to reiterate, are meant conscious beings, unique and complex totalities, identical yet changing, related to their environment—the distinctive beings, indicated by such expressions as "I am disappointed in myself," "I envy you," "I admire him."

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[74] Note 33 to Vol. II., Chapter XIV, Section 7 of James Mill's *Analysis of the Phenomena of the Human Mind.*

[75] *A Beginner's Psychology,* 1915, § 40, p. 186.

I come back accordingly, enriched I hope by the intellectual spoils of all these years, to the position long ago attained. The most important present task of systematic psychology seems to me to demand the acceptance of personalistic psychology in one of its forms; and to include the establishment, by experimentally controlled investigations and by seriously undertaken non-experimental observations, of the basal categories of psychology thus conceived. The decisive reason for this conclusion consists simply in the intellectual necessity of fitting the basal concepts of psychology to the basal facts of introspection; and a second significant, though subsidiary, reason is to be found in the present-day prominence of the social and of the therapeutic sciences. Sociology and political science, mental hygiene and psychotherapy, are fundamentally psychological disciplines; and the psychology which lies at the root of them is indubitably social psychology. But social psychology obviously is self-psychology, for it presupposes the existence of selves in relation to each other and indeed consists precisely in the study of these selves as variously related. To substantiate this claim it is necessary only to cast a glance at the intrepid but wholly unsuccessful efforts of behaviorists to deal with social phenomena. Impersonalistic introspectionists, conceiving their science as the study of successive psychic events, for the most part ignore the concrete problems of social psychology. Behaviorists, on the other hand, have much to say of social behavior, "the reactions to language, gestures, and other movements of our fellow-men," as opposed to non-social behavior, namely, "our reaction toward non-social objects such as plants, minerals, tools."[77] Weiss, for example, who maintains that "all human conduct....reduces to *nothing but* different kinds of electron-proton groupings" and "the motions that occur when one....form changes into another,"[78] none the less stresses the distinction between 'me' and 'my fellow-man';[79] and Watson, though he "can get along without consciousness,"[80] urges the following questions (among many others) "as indicatory of.... factors which we should have information about whenever there is practical or scientific need for a personality judgment. . . . Is [the subject] loyal to his friends? . . . Does he sacrifice his work and responsibility to his supporting tendencies?....Is he affectionate and kind or jealous?.... Is he domineering or submissive?....Is he truthful, faithful to his word?....Is he easily shocked?...." With superb inconsistency these behaviorists overlook the fact that loyalty and responsibility, jealousy and kindness, domination and submission, truthfulness and being shocked, are not the qualities of bodily processes nor of electron-proton aggregates. To state this criticism more generally: on the behavioristic theory, no distinction is possible between social and non-social behavior and its objects. For the behaviorist conceives psychology as the study of reacting bodies, that is, of moving physical objects, and from this point of view there can be no basal difference between a human being and a plant or a tool; all are alike moving bodies. In a word, the behaviorist has no right to the conception of "the individual and his fellows," for by 'fellow' he must mean precisely a being conscious, like himself, with whom he is in realized relation. I am brought back in this fashion to my inital assertion that social psychology is inevitably personalistic psychology. And this drives home the conviction that a scientific pursuit of personalistic psychology is imperatively needed today for the grounding and the upbuilding of the still unsystematized and eclectic disciplines roughly grouped as the social sciences.

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[76] *Textbook of Psychology,* Part II, 1911, p. 467. Note.

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[77] Allport, F. H., *Social Psychology,* p. 3. It is unnecessary to add that Allport includes within social psychology the study of consciousness accompanying social behavior.

[78] *A Theoretical Thesis of Human Behavior,* 1925, p. 50^1.

[79] *Op. cit.,* p. 288 and *passim.*

[80] *Op. cit.,* p. VIII.

EDOUARD CLAPARÈDE*

A man's life may be compared to a flowing river, each has its own individual course. How could the psychologist remain indifferent to its winding curves, the countries it traverses, the shores its waters bathe and fertilize? I do not hesitate therefore to set down here the story of my life, though it is as empty as possible of any dramatic or sensational incident. My little stream, which took its rise in a lovely garden, has had but to follow the easy slope of a country cleared of all obstacles. I ought perhaps to blush at having produced so little in such favorable conditions.

My family is Protestant and came originally from the Languedoc (France) and settled in Geneva after the Revocation of the Edict of Nantes. Curiously enough, for a citizen of this Geneva—situated at the crossroads of the Latin and Teutonic civilizations—I have not a single ancestor of Germanic descent, not even a Swiss-German. So far as I know, they have all been French or Italian refugees or autochthons from the French part of Switzerland. I mention this, because, though I have sometimes joked at the *"Grundlegungstrieb"* of German psychologists, yet I feel very akin to them by a certain aspiration I have for *"Gründlichkeit."*

There has not been a single man of science among my ancestors, who were nearly all clergymen or magistrates. Three collateral relations only, an uncle, a cousin, and a brother, took up the exact sciences. My father was a clergyman, as his father had been before him. But when I was born, he left his parish to devote himself to the study of the history of Protestantism. He was a modest, simple man, rather shy, very conscientious and sure in his work. My mother was also modest, kind-hearted, and of a sweet temperament. I was brought up without the slightest trace of severity—I was, indeed, rather a good child by nature, I think. Born in Champel on March 24, 1873, in the old house in which I still live today, I spent my first years playing all day in our big garden, which then seemed to me a vast world. It was for me the Garden of Eden itself, and I sincerely believed that Eve had tempted Adam at the foot of a certain peach tree, which I could still show you today. We had then a vineyard, and a wine-press, and a stable with cows. In the summer, I helped, or thought I did, to make hay with a little pitchfork. I climbed on the cherry and plum trees to enjoy their fruit. In a word, from my earliest childhood, this contact with nature made me deeply happy. I loved the smells of grass, wood, earth, the scent of flowers, and this attraction of country-life has never left me. I was thus the happiest child one could imagine, at any rate till I went to school, at the early age of four and a half!

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*Translated from the French by Miss D. Beineman.

How is it that, starting from this kind of life, I found my way to psychology? Certainly the first job which appealed to me was not that of psychologist. I remember having wished to be a postman and then a coachman; more especially the latter. One of my favorite games was to perch with a long whip on a stool placed on a table and drive a team of four or six chairs which represented galloping horses. When he tells the story of his life, the psychologist, alas, cannot allow his memory simply to retrace past events! He thinks himself constantly obliged to "explain." I do not know whether this game of coachman deserves a special explanation. Children have always loved vehicles, today they are keenly interested in motorcars and aeroplanes. In 1876 we were still in the age of fine carriages and big stagecoaches with the postboy blowing the horn on entering the village. There is nothing strange in the fact that a little boy of that day should have imitated the high-perched driver of these splendid turnouts.

And yet, now that the inferiority complex is the fashion, I wonder if this passion for "driving," for "leading," was not the expression of some "compensation." I was the youngest of five children. My eldest brother was fifteen years older than I, the sister I came next to, eight years older. I was the "little one" of the family. I remember quite well that this name of "little one" by which I was persistently called exasperated me considerably, somewhat later, when I was ten or eleven. Add to this that I was often compared to a little cousin, three months my junior, but taller and stronger than I, and that I found this humiliating.

It was therefore one of the joys of my childhood to be elected corporal and then lieutenant (the highest rank) at the *École privat,* which I attended from my eighth to twelfth years. The pupils of this school were organized in military fashion, and I was very proud to be in a position to order my comrades about, all the more so as I had then but feebly developed biceps. This revenge of gold-braided and plumed authority over muscular weakness was not without savor.

This militaristic period of my childhood, during which the only presents I desired were epaulets and military caps, guns, and drums, now seems to me very strange. With the passing of the years, I have completely lost this desire for authority—at any rate for that kind of authority, exercised sword in hand. All Swiss citizens are obliged to serve in the army. I served as Medical Officer. But, if I did my duty honestly, I never felt within me that mentality of the officer who likes to make his subordinates conscious of his superior rank, and my superior officers often blamed me for never "commanding."

I must note, by the way, that the fact of having been, as a child, so passionately fond of military games has in no wise made me a militarist. I have as little of that attitude as is possible. Yet I must admit—such are the strange contradictions of the human soul—that I have always experienced a very deep emotion when I have seen the flag pass at the head of a battalion. In the same way, though I am a fervent internationalist, yet I have remained an ardent Genevese and Swiss patriot.

I must here set down an event which doubtless had some influence on my career.

Two years before my birth, a brother of my father died at the age of thirty-nine. He was a naturalist of great distinction, exceptionally gifted, well-known by specialists for his remarkable studies of infusoria and annelida. He had been a pupil of Joh. Müller in Berlin, at the same time as Haeckel, and was the first, on the continent, to spread Darwin's ideas. A dangerous polemist, he it was who wrote the famous sentence: "I would rather be an evolutionally developed ape than a degenerate Adam." This Uncle Edouard, who had made such a mark, was often spoken of in my family. I had been given his name, and when I heard people praising "Edouard Claparède" it stirred up a strange sensation in my little head—I was then three or four years old. It was as if one spoke a little of me or, at any rate, as if some special tie existed between me and this celebrated man (magic virtue of the name!) and that it was my duty to bring honor to this name which he had distinguished so brilliantly.

Thanks to this sort of identification, my next ambition, when my aspiration to the high calling of coachman had faded away, was to become in my turn a famous zoölogist. I still have the little notebooks in which I scribbled when I was six or seven and naïvely copied skeletons of man and animal, as an initiation to my future career.

School with its overcrowded programs kept me from indulging in my taste—real or imaginary, I do not quite know which—for natural history. As a matter of fact, I never collected insects or butterflies or shells, and took no interest in the cultivation of my little garden. I much preferred "making" things, building anything, a hut in the top branches of a tree, boxes for my guinea-pigs, little benches for the garden, or repairing furniture, sawing, hammering—hammering especially. The only thing that I ever did collect with interest for any length of time was stamps. In short, I preferred action to observation. And yet, when I say "action," I must qualify this statement. I have always desired action, it is true. But this action has nearly always taken place in the realm of thought. It has always been difficult for me to pass to outward realization, as if I were struck by some mysterious incapacity just when I had to act.

What a curious contrast between the wish to do a thing and the capacity really to execute it! My whole childhood was spent—apart from games of all sorts, which I passionately loved—more in elaborating plans, than in realizing them. I was very fond of drawing. Stimulated by the example of Toepffer's albums of caricatures (well-known Genevese author who illustrated his own stories), how many times did I not begin a story in pictures! But it always stopped at the third page—when it was not at the second! The same thing happened with the Punch and Judy shows I wanted to get up, or the tales of adventure I proposed writing. A beautifully written title, a few pages, and that was all. The reason was that I was never satisfied with what I had put on paper. There was such a disproportion between my dream and its realization that I was at one and the same time disgusted and discouraged.

For this same reason I gave up the violin after two or three years' study. Though I was not much of a musician, yet I had enough musical feeling to find the deplorable screechings of my instrument unbearable.

My disposition is unhappily still the same. I am quicker at sketching out a plan of work than at executing it. My drawers are full of work begun which has not yet seen the light of day, and will certainly never see it—a book on animal psychology, announced long ago in Ebbinghaus' collection; another book, *Education and Interest,* of which three chapters were written in 1914; still another, on *The Will,* fifty pages of which have been ready since 1924; and all the continuation of my *Psychology of the Child,* the one volume of which has at present been published was to have been just the introduction. . . .

As I was saying, my ambition was then to become a famous naturalist, and this, in spite of my father, who wished me to be a clergyman. However, Jules Verne's books and those of Mayne Reid, more especially, of which I was very fond, gave me a great desire to explore the world, and for a time, I dreamt of nothing but the Pampas plains, desert islands, wild Indians. I made myself a bow and arrows and practiced shooting at red Indians, full-sized, cut out of cardboard. I combined this new wish quite easily with my former ambition, and declared I'd be a "naturalist-explorer."

At this point my father died suddenly. I was hardly fifteen. This unexpected death turned my thoughts to serious matters. A novel I read at that time, which was about a very pious young girl, if I remember correctly, determined a sort of religious crisis in my life.

My parents were sincerely religious-minded, but discreetly, and without any ostentation. I obediently attended Sunday School. Though it did not excite my enthusiasm, yet I was interested and liked it. It has certainly contributed to my moral development, but my religious feelings were in no wise fanatical. After reading this book, however, I suddenly felt I wanted to devote myself to the propagation of the Gospel. I would thus be fulfilling my father's wish.

But this did not cause me to abandon the idea of being a man of science and an explorer, and I reached a synthesis of all these different aspirations in the new formula "medical-missionary."

For some time after this, I took missionary papers and read them assiduously.

But once again the direction of my plans was modified by a new incident. I made the acquaintance of a young foreigner, of Russian origin, daughter of the philosopher, African Spir. The very day I saw her for the first time I decided none other should be my wife. But I was only fifteen and a half, and to ask for her hand then was out of the question. The following year, in 1890, her father died. Unfortunately I never had met him. She left Switzerland and I very nearly got out of touch with her. That was enough to make me lose any desire to explore the Grand Chaco or evangelize the Bassoutos, for I knew that Helene (this was my sweetheart's name) was not strong enough to follow me in these adventures!

I got through my last years of college with impatience on account of the heavily overcrowded programs. I changed from the Classical Section to one where the natural sciences were better represented. I was a rather good pupil and could not help feeling some pity for those of our masters who were the butt of their pupils' naughtiness.

In 1891, at last, I left college, and soon after I published a little pamphlet *A Few Remarks on the College of Geneva,* in which I ventured to ask for a reform of the methods in use in "Calvin's venerable institution."

Was the future naturalist, doctor, explorer, and missionary going to be simply a pedagogue?

No, he was not thinking of that at all, he was preparing to start his medical studies, as an introduction to natural science. His uncle had followed this course, and also his cousin, Theodore Flournoy.

I must now introduce Flournoy, a delightful man and keen thinker. He was the son of a sister of my father and nineteen years older than I. As he was of a rather retiring nature, and preferred the quiet of his study to our family parties, one did not often meet him, and because of the difference of age between us, we hardly knew each other. But in 1886, at our University, where he was *privat-docent,* he gave some lectures on Kant, which were much discussed. His depth and clearness of thought were commented on very favorably.

At that time, natural science was taught in the Classical Section of our college by a Latin master who made us read and recite a dreary textbook. There was no laboratory work, and I, the budding naturalist, had but the vaguest notion as to what a "cell" is. One fine day, I suddenly wished to know something more on this subject, so I went and rang with some trepidation at the door of my distinguished cousin Theodore.

He received me most delightfully, showed me microscopic preparations of the cells of an ivy leaf and told me to come again and see him as often as I wished. The ice was broken, but I did not take too great advantage of his invitation. However, two years later (1891) I attended a public lecture he gave at the University on

"The Soul and the Future of Psychology." This was my first introduction to psychology, and it was the new psychology, that of Fechner and Wundt. Flournoy's audience listened with surprise to his talk of laboratories of psychology (a brand new expression, which one hardly understood then), of the delicate accuracy of measures, of stop-watches giving records to the 1/1000 of a second. He concluded his lecture by wondering if, in Geneva, the town of Charles Bonnet, where honor had always been paid to the exact sciences, men would not be found to come forward and work in this new field.

Hardly had these words struck my ears, pronounced as they were in that tone of conviction which Flournoy used in his affirmations, than I had the very definite impression that I had at last found my vocation.

But I still did not give up medicine, which seemed to me the best introduction to the study of man. I spent a year first at the Faculty of Sciences of Geneva University, where Carl Vogt taught zoölogy. This year remains in my memory as one of the most delightful of my life. Once out of that hated College, I was free to work according to my tastes and discovered at last the joy of work. Botanical or zoölogical excursions brought me into touch with reality, with Nature I loved so much. I became acquainted with Science, I entered more and more, though somewhat shyly, into her intimacy, as if she had been some lovely distant lady, long desired. This was far indeed from what our college textbooks had led us to understand! I discovered then that Science is keenly alive and mobile, eluding one's grasp, always progressing and ever having to be conquered. In short, I was completely fascinated: the peculiar *sui generis* odor of the laboratories seemed to go to my head.

In the same year in which I began my studies in the Faculty of Science, a regular course in Experimental Psychology had been established, and a Laboratory. Flournoy, the new Professor, had asked of the State of Geneva that this Laboratory be included in the Faculty of Science and his request had been granted. This marks an important date in the history of psychology, for it was the first time this science was officially detached from philosophy and given its proper place.

I naturally attended Flournoy's lectures and took the practical work in his Laboratory. Three small rooms only, in the basement of the University, had been allotted for this Laboratory. They had something rather mysterious about them. We were there half a dozen neophytes, occupied in taking reaction-times and determining sensitivity thresholds, without understanding very clearly, I must confess, the meaning of these experiments. It was all so new! One day William James paid us a visit at the Laboratory. Flournoy was very fond of him. James told me how struck he had been formerly with the teaching of my uncle, the naturalist, whose pupil he had been in Geneva, in 1859. The name of William James was very familiar to Flournoy's students. He often mentioned him in his lectures and had adopted his theory of emotion.[1] When I heard this theory developed for the first time, far from thinking it strange or unacceptable, as appears to be the case with most people, I at once found it self-evident and corresponding to a very familiar personal experience. I can even say I had discovered it all alone, when I was still a child. However I do not claim any rights of priority, for the incident I am going to relate took place in 1885, whilst James published his memoir in 1883! But it seems to me typical enough to be related here, though I must apologize for its triviality.

It was in the autumn of 1885. I was twelve, and was standing for the first time in the playground of the College at Geneva, waiting to go in for the entrance examination. As may be guessed, I was rather nervous, and this caused me to repair frequently to a certain little room. "How scared you are!" said my friends, laughing, when they noticed these oft repeated trips, "It makes you pass water all the time." "Why, no," I answered, "it's not because I am scared that I must do that, but it's this constant need which annoys me and makes me scared." My little friends laughed and thought me paradoxical, but since then I have often repeated the same experience, and that in cases where the emotion I felt could absolutely not be explained by motives of an intellectual order. This theory of James and Lange, which has always appeared to me as clear as daylight, from the theoretical as well as from the experimental point of view, has constantly been a precious guide for me for the understanding of a number of psycho-physiological phenomena.

Having heard about *audition colorée,* I was very surprised to learn that this phenomenon, which was present in me to a marked degree, is an individual peculiarity, and that Flournoy, for instance, saw no color corresponding to vowels. I then conceived the idea of inquiring into the frequency of this phenomenon. Flournoy encouraged me and I distributed a questionnaire among the university students and my friends (May, 1892). I heard that Binet was studying the same problem, so I went to Paris and paid him a visit. This was the first time I met him. He congratulated me on my undertaking and asked me to send him my first results. I did so, and he mentioned them in an article on *audition colorée,* which he published in the *Revue des Deux Mondes* of October, 1892, where he called me a "distinguished psychologist." This flattering title, the exaggeration of which I was somewhat uncomfortably aware of, served, however, but to confirm me in my vocation: I was to be a psychologist . . . . it was printed black on white!

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[1] Flournoy made use of this theory to explain most cases of synopsia (see *Des phénomènes de synopsie,* Geneva, 1893, p. 22).

The questionnaire brought some hundreds of interesting answers. But, alas, the "distinguished psychologist" was too much of a beginner in psychology to turn them to account. Besides, I left that autumn for Leipzig to begin my medical studies. So Flournoy published the results of this work in his fine book on *Synopsies* (1893).

In Leipzig I worked in the laboratory of W. His, and attended Ludwig's lectures. I had also put down my name for a "Praktikum" of psychology, which Külpe then gave in Wundt's Laboratory. Unluckily, I was the fifth on the list, and Wundt had taken it into his head that only four students should attend this course. So, after my first two attendances, I was obliged to leave, in spite of Külpe's intervention in my behalf. I have always regretted it.

I spent only one semester at Leipzig, as I did not want to be away from my mother longer than that. All my brothers and sisters had married and she lived alone in our old family house at Champel. So I finished my medical studies at Geneva and obtained my M.D., in 1897, with a thesis on *Du sens musculaire à propos de quelques cas d'hémiataxie posthémiplégique.* This brought me back to psychology, which I had abandoned four years before. The study of a hemiplegic case who had ataxia of an arm led me to the study of the muscular sense.

Meanwhile, I had been a very active member of the Society of Zofingue, which is composed of students from all over Switzerland.

I was Central Secretary for a year and, as such, edited the monthly review, which satisfied my thirst for activity.

In 1896 I married Helene Spir, to whom I had remained faithfully attached since 1889. In 1898 my wife and I spent a year in Paris, where Dejerine consented to accept me as a worker at the Salpêtrière; I there pursued my studies of ataxia and the disturbances of sensitivity and drew sections of the brain in the little laboratory. But I must admit that, in this laboratory, the "Dreyfus Case," which was then at its acutest point, was discussed much more than the problems of neurology. Dejerine was ardently anti-Dreyfus, while all his house-internes and students were as ardently pro-Dreyfus. So we came to grips every day over this burning question, though most courteously, with the master we all admired immensely but could not follow in his political ultra-nationalism. Dreyfus was then on Devil's Island, chained by each ankle, and the whole of France, the whole of Europe, was in a state of wild excitement. "If Dreyfus were really a patriot," Dejerine used to say, "he could plead guilty, even though he were innocent, so as to end this agitation, which is so disastrous for France." If I recall this, it is to show to what lengths the logic of sentiment may carry a man as distinguished and kind as Dejerine was.

At the Salpêtrière I did research work on the disturbances of sensitivity in ataxic and hemiplegic cases. I also studied stereognostic perception and its disturbances, agnosia and apraxia.[1a] Neurology thus led me back to psychology.

During this stay in Paris, I became an intimate friend of Binet, who used to be at home every Thursday in his laboratory. On other days he experimented in schools. I there met Victor Henri and my compatriot, Larguier des Bancels. Henri was then also studying the muscular sense. We began together a vast experimental study of the representations of movement and the qualitative difference, in introspection, between passive and active movement. But we intended to complete and improve this work—and never published it.

My wife and I also had occasion often to meet the Gleys, the Marilliers, and the families of the philosophers Boutroux, Xavier

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[1a] See "La perception stéréognostique et la stéréo-agnosie." *Année psychol.*, 1899, **5**; *J. de physiol. & de pathol. gén.*, 1899; and the medical dissertation done by my pupil Miss Markova, *Contribution a l'étude de la perception stéréognostique.* Geneva, 1900.

Léon, Rauh, Brunschvicg, and others, who have become our good friends.

On returning to Geneva, I was delighted to follow Flournoy's clear and enlightening teaching once more and started to work in his laboratory. From 1899 on I gave in this laboratory a course of practical exercises, as *privat-docent,* on the sensations. However, I had not decided to give up neurology and I followed various cases of nervous diseases at the medical and psychiatric clinic, where, from time to time, I replaced the house-doctors during their absence. For a few years, I had a consultation at my home for psychoneurotic cases and a free consultation of psychotherapy at the medical dispensary; these I continued until 1920. An almost miraculous cure of a serious case of morbid blushing encouraged me to persevere in my line of work.[2]

Meanwhile my interest in zoölogy was still as keen as ever, and I would have liked to start work on animal psychology. But Loeb's *Vergleichende Gehirnphysiologie* (1899) and various articles of Bethe, where the possibility of animal psychology was denied, led me to take up the line of criticism and theory, and I published some articles to demonstrate that animal psychology was quite as justifiable as human psychology.[3]

Psychologists have lost much precious time in these controversies about the limits and possibilities of psychology. Has any one adversary ever been convinced by the discussions which the question of behaviorism has given rise to in the United States during the past twenty years? Besides, these discussions have been a mere repetition, at a quarter of a century's interval, of those which took place in Europe between Richet, Binet, Gautier, Herzen, Soury, Flournoy, Forel, and others. If one leaves out a few differences of terminology, one sees that the arguments brought forward on the one hand or the other are, at bottom, exactly the same. For my part, in these discussions I adopted Flournoy's point of view of parallelism, not as a metaphysical principle—he declared that parallelistic dualism had never been asserted in philosophy[4]—but as a methodological principle.[5] While it is a scientific expression of the close union which exists between processes of conscience and cerebral processes, this principle also has the great advantage of removing all sterile discussion as to the nature of this union. It enables psychology and physiology to remain in close harmony with one another.

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[2] See "L'obsession de la rougeur, à propos d'un cas d'éreutophobie." *Arch. de psychol.,* 1902, 1.

[3] "Les animaux sont-ils conscients?" *Rev. phil.,* 1901. "The consciousness of animals." *Int. Quar.,* 1903, 8. "La psychologie comparée est-elle légitime?" *Arch. de psychol.,* 1905, 5. See also my book, *La psychologie animale de Ch. Bonnet.* Geneva, 1909.

[4] See Flournoy's "Sur le panpsychisme." *Arch. de psychol.,* 1905, 4, 137-138.

Thus in 1900 I was engaged in three lines of activity, clinical neurology, laboratory research work, and animal psychology. But now my attention was drawn to a new field of work, that of psychology applied to education.

Special classes for backward and subnormal children had just been started in Geneva. But the teachers had received practically no special preparation and felt rather at a loss. They came to me for advice. I did not know much more about it than they did. So I began by visiting their classes and acquainting myself with the question. A visit to Brussels, where Demoor and Decroly were working on the same problem, was of great value to me. And so I was able to give these ladies the few lectures they required. Then the Education Department of Geneva asked me to send in a report on the teaching of subnormal children and the improvements needed. All this stimulated my interest in pedagogical psychology and I was led to notice once again the defects of existing school systems, though from another point of view this time—that of the doctor and not that of the pupil. And so, in 1901, at a lecture given at the Medical Society of Geneva, I put forward a claim for "l'École sur Mesure"—the school made to measure.[6] This formula, which then seemed to express a wild Utopia, is now accepted readily by educationalists themselves as setting forth an ideal to be realized.

As has been seen, I wandered from right to left, trying to work in every field of psychology at the same time. I was afraid to specialize, for I felt vaguely that it would hinder me from having a clear and complete view of mental life and its mechanism. I had no guiding principle which could give me this *vue d'ensemble.* On the one hand, my neurological studies led me to reduce mental activity to the activity of a constellation of cerebral centers, more or less definitely localized; on the other hand, the most elementary observation of the phenomena of human conduct showed me how impossible it is to bring its manifestations, in their infinite variability, into the rigid framework of this clumsy scheme.

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[5] See Flournoy's *Metaphysique et psychologie.* Geneva, 1890, new ed., 1919. See also my article on "Th. Flournoy." *Arch. de psychol.,* 1921, **18**, 2.

[6] See *Rev. méd. de la suisse rom.,* 1901, p. 608: "We are not," I said, "as careful of our children's minds as we are even of their feet. Shoes are made of different sizes and patterns to suit their feet. When shall we have schools made to measure?" See also *L'école sur mesure,* Lausanne, 1920.

This situation was far from comfortable. I am of a markedly visual type. I must *see* in order to understand; I can only grasp what I can visualize in space. But I would rather not understand than arbitrarily adopt simple schemes which do not faithfully reflect reality.

It was at this point that I had the good fortune, in 1900, to come across Karl Groos' excellent book, *Die Spiele der Tiere,* which opened up new horizons to my mind. It showed me the help animal psychology could give to human psychology, drew my attention to the importance of instincts in mental life, and revealed to me, as in one illuminating flash, what should be the foundation of the art of education, i.e., the right use made of the child's natural tendencies, and, more especially, of the play-tendency. It is due to this book also, I think, that I exchanged my too narrow physiological and cerebral conception of psychological phenomena, for a biological conception, wider and more dynamic, which henceforward has been as the thread of Ariadne in my work.

At this time Dr. Toulouse, who had undertaken the edition of a series of books on experimental psychology in Paris, asked me to write the volume of this series which was to deal with the "Association of Ideas." I set to work, and the book was published at the end of the year of 1902. In its pages I attacked the theory of association, which may have seemed rather daring on the part of a young man, a newcomer in psychology, as this theory was then tacitly accepted by all psychologists.

Of course, I knew too well the great importance of association in mental life to deprive it of its rightful place. My purpose was to set forth "what association explains and what it does not explain." What it does not explain, in particular, is the direction of thought, the existence of the different forms of association. Before writing this book, I experimented on a large scale with predetermined associations (a word being given, the subject must answer with another word, which is associated with the first in a relation of, e.g., causality or subordination, etc.). I had noticed that association was very often accompanied by "the feeling of the direction in which the answer will be given," that *feelings of relation* crept in between the word used as the stimulus and the word given in response.[7] These introspective experiments brought me to the threshold of that region which the psychologist of the Würzburg School were to explore a few years later. But it was only with timidity that I indicated my discoveries. I did not dare to dwell on results which seemed to me too much outside the limits of current psychology, and I declined to believe rather that my inexperience hindered me from fitting these results within the usual limits.

But then, how were intelligent action, direction of thought, to be explained? It is here that the biological conception threw light upon my road. It did not give me a mechanical explanation of this *adaptation to the situation of the moment,* which is the characteristic of mental activity, but it furnished me with a new vision of the dynamism of conduct and suggested that I should look beyond—if I may be allowed the expression—the cerebral process for its origin, i.e., in that relation between the stimulus and the need of the organism which is indispensable for the maintenance of life. How is one to explain this correspondence between a need and the adequate reaction, capable of satisfying it? Or, in other words, how is one to explain this capacity of the organism to react according to its interest of the moment? Can one explain it mechanically? That is an ultimate question which can be disregarded, it seems to me, for it concerns biology and not psychology. For psychology this capacity for *adequate* reaction is a given fact which it is not called upon to interpret, anymore than it need interpret the phenomenon of life.

This fundamental property of mental activity (that of serving the biological interest of the organism) has seemed to me worthy of being called a law: the *Law of Momentary Interest.* "At any given moment, that instinct which is of greatest importance takes precedence over the others," or, "at any given moment, an organism acts according to its strongest interest."

When I put forward *interest* as the underlying cause of the adaptation of conduct, I never thought of considering it as a really explanatory principle, a kind of *deus ex machina,* taking the place of the soul or of apperception in the psychology of former days, and this without any advantage. I wished simply to express a general fact, a relation between need and reaction, which can be observed constantly (except in those cases where it is accidentally perverted), something like the Law of Gravitation in the physical world, which expresses a fact often veiled also by disturbing circumstances—without, for all that, claiming to explain its ultimate mechanism.

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[7] See *L'association des idées.* Paris, 1903, pp. 229, 350, 366, etc.

Physiologically, this "reaction of interest" can be represented as the *dynamogenization* of the processes of reaction adapted to the situation of the moment. This *dynamogenization* is determined by the stimulus together with the need of the moment. The reaction is *dynamogenized* in proportion to the degree in which the stimulus is capable of satisfying the need.[7a]

In my book on association I also considered the phenomena of judgment and feelings of relation as reactions of the organism or as the consciousness of these reactions. Thus I substituted a reaction of the organism—a reaction which has ultimately a biological significance for conduct—for the purely cerebral explanation (as, for instance, that of Ziehen, who held that in the judgment, "the rose is beautiful," the copula "is" is but the psychological correlative of the cerebral process which unites "beautiful" to "rose."

In the phenomenon of sleep, I found a striking instance of the fecundity of the biological interpretation and of my Law of Momentary Interest. I must here relate how I came to study sleep: It was by mere chance, unless one cares to invoke subconscious rumination. It was in 1903. I was giving a course of lectures at the University on animal psychology. We had just begun the study of instincts, and I enumerated various instincts, those of nutrition, protection, sleep. . . . Hardly was this last word out of my mouth when I had the impression that I had said something very foolish. This bothered me till the end of my lecture. As soon as I got home, I hastily opened my books of physiology to see what they had to say on the subject, for I had never taken any special interest in the phenomenon of sleep, nor had it ever been mentioned to me during the years of study.

I was, first of all, astounded to see how neglected this question was in all treatises of physiology and psychology. In many of these it was not even mentioned, and those that did mention it, considered sleep as the result of an intoxication. At first, I was quite cast down at the idea of having, in my lecture, called instinct what was but a kind of asphyxia or paralysis. . . . But, on further thought, the toxic conception soon appeared to me a naïve absurdity, and ideas crowded into my mind to justify what I had feared was a silly slip of the tongue. Yes, sleep is a *positive action,* a *reflex,* an *instinct of protection,* which is not the result of intoxication, but whose function, on the contrary, is to prevent the organism from reaching the point of exhaustion. We do not sleep because we are intoxicated, we sleep so as not to become intoxicated.

[7a] I presented my conception of interest at the International Congress of Psychology at Rome in 1905 (*Atti del V. Congresso*, p. 253). In order to explain *dynamogenization,* I offered the hypothesis of a "reservoir of energy" (*Arch. de psychol.,* 1905, p. 56), quoted also in my *Psychologie de l'enfant,* 1909, where I further applied it to the processes of fatigue.

When Freud's work appeared, I thought that the best way to explain his *libido* would be to identify it with "interest." But Freud did not agree. See my "Introduction" to the first French translation of Freud, done by my assistant Le Lay, and Freud's reply, *Cinq leçons sur la psychanalyse,* Paris, 1924.

This function of protection consists in cutting off the individual's interest in the situation of the moment, and thus stopping his activity. But this psychological "dis-interest" is itself due to an organic interest, and sleep can, therefore, be considered as one particular case of the Law of Momentary Interest. This biological theory of sleep has the advantage of suggesting new problems, such as that of the phylogenetic origin of sleep, or of throwing light on the *raison d'être* of certain facts, such as, for instance, autism in dreams.

This conception met only with amused skepticism, when I spoke on this subject before the Society of Physics and Natural History of Geneva, in February, 1904, and, a little later, before the first German Congress of Psychology at Giessen. Flournoy alone was encouraging. I then developed my theory in a long article.[8]

Today, after a quarter of a century, I see that, in most of the recent publications on sleep, the biological conception is adopted or that documents are brought forward in its favor.[9]

This study of sleep has remained my favorite subject of work. Doubtless because I feel it is the only truly original one.

The problem of sleep led me to that of *hysteria,* for which I also proposed a biological conception:[10] hysterical manifestations, such as anaesthesia, amnesia, syncope, etc., would thus simply be reactions of defense, equivalents of which are found in animals. This conception was much criticized by Babinski and others at the Congress of Neurologists of Geneva-Lausanne, in 1907. But it was taken up again—sometimes in the identical words—by Kraepelin in 1915, and Kretschmer in 1923 (who, however, did not mention me).[11]

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[8] "Esquisse d'une théorie biologique du sommeil." *Arch. de psychol.,* 1905,

[9] "Opinions et travaux divers relatifs à la théorie biologique du sommeil *Année Psychol.,* 1912, **18.**

[9] "Opinions et travaux divers relatifs à la théorie biologique du sommeil et de l'hystérie." *Arch. de psychol.,* 1928, **21.**

[10] "Quelques mot sur la définition de l'hystérie." *Arch. de psychol.,* 1907, **7.** "The value of biological interpretation for abnormal psychology." *J. Abn. Psychol.,* 1906, June.

The conception of interest has also been my criterion to distinguish an "action," a "conduct" from any other kind of movement of an organism, such as, for instance, the tropisms in the sense in which Loeb uses the word. An action, a spontaneous reaction (as opposed to the tropism or the simple, mechanical reflex, such as the patellar reflex) is any reaction governed by the Law of Momentary Interest, which law adapts itself to the varying needs of the organism (the stimulant remaining the same).[12]

The further I advanced, the richer did this biological conception appear to me. I chose by preference the expression *functional conception,* as it considers psychical phenomena primarily from the point of view of their function in life, their whole place in the "ensemble" of conduct at any given time. This comes to the same thing as asking one's self: What is their use? After having thus asked myself: What is the use of sleep? I examined in the same way the use of childhood, of intelligence, of the will. . . .

This form of speech has naturally been much attacked. I have been accused of mysticism, finalism, even Calvinism! But quite wrongly. No one is more decided than I am to keep on the firm ground of experience in psychology, and I have always stood up for a genuinely scientific psychology, clearly separated from philosophy. But just because I am a determined empiricist, I cannot shut my eyes to the fact that certain processes are *useful* for the maintenance of life. Does one fall into mysticism if one asks what is the use of the pancreatic juice or the red globules of the blood? I believe, on the contrary, that we show our confidence in science when we bring within her jurisdiction the question of knowing how these useful functions have developed in the organism. But, if one would examine this problem, one must not begin by shutting one's eyes to it, in the name of some dogma, borrowed from extra-scientific considerations. This ostrich-like attitude is not worthy of the man of science, who should be capable of examining everything, without any preconceived ideas.

Besides, the functional point of view can be expressed in a form less offensive to the ears of the positivist. Instead of saying: "What is the use of intelligence?" one can ask: "What are the circumstances which determine the intervention of intelligence?" I have attempted to show that intelligence intervenes when instinctive or acquired automatism is not capable of solving the problem which confronts behavior, and I have derived intelligence from the method of trials and errors of inferior animals. But, in the case of intelligence, the problem of readjustment to a new situation is solved by thought.[13] My definition thus coincides exactly with that of Stern.

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[11] See in my article mentioned above, *Arch. de psychol.,* 1928, the passages of Kraepelin, Kretschmer, and other authors.

[12] "Les tropismes devant la psychologie." *J. f. Psychol. & Neur.,* 1908, **13**.

The same question applied to the will showed me that just as intelligence has to solve problems of means so the function of the will is to solve problems of ends. It comes into play when action is momentarily suspended by the conflict of two groups of tendencies, and readjusts action by giving the supremacy to the higher tendencies.[14]

This same functional point of view seems to me to throw some light also on the confused problems of feelings and emotions.[15] Even the problem of the presence of conscience is illuminated by the "functional question."

Experiments with children have shown me that the consciousness of resemblance appears later than that of difference. Yet the child, from a very early age, behaves as if it perceived resemblance. The recognition of this fact led me to formulate the Law of Becoming Conscious (*Loi de prise de conscience*): The earlier and the longer a relation has been in use, the later it is consciously perceived.[16] Thus consciousness intervenes when action is obstructed—one can say that the development of mental life is proportional to the width of the gap between the needs and the means of satisfying them. The function of mentalization is evidently to relate those processes, which had been unconscious till that moment, with the representations which are due to acquired experience, and thus to permit the readjustment of action. But we do not know the part played by consciousness as such. Is it simply an accompaniment of energetic cerebral processes, or has it a peculiar quality of its own, as vitalists and interactionists believe? I abstain from giving a definite answer to this question: on the one hand, mechanism appears to me incapable of explaining the phenomenon of adaptation to new problems; on the other, the vitalist's explanation seems merely verbal and does not satisfy my desire for mechanism. And I see no issue from this dilemma.

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[13] "Tierpsychologie," in *Handwörterbuch der Naturwissenschaften,* IX, Jena, 1913, p. 1198. "La psychologie de l'intelligence." *Scientia,* 1917.

[14] "La définition de la volonté." *Int. Cong. Phil.,* Naples, 1924. "Does the will express the entire personality?" Problems of Personality, Studies in honor of Morton Prince, 1925.

[15] *Feelings and Emotions: the Wittenberg Symposium.* Worcester, Mass.: Clark Univ. Press, 1928. See p. 125.

[16] "La conscience de la ressemblance et de la différence chez l'enfant." *Arch. de psychol.,* 1918, 17. Needless to say, this law has nothing to do with the metaphysical problem of the body and the soul. The "awareness," this "mentalization" can be represented, in accordance with parallelism, as corresponding physiologically to a "corticalization" of cerebral processes.

The Law of Becoming Conscious enlightens some processes of the act of intelligence. I have distinguished in each complete act of intelligence three fundamental operations: the *question,* the *hypothesis,* the *verification.* Now the "question" is nothing else than the becoming conscious of the difficulty to be solved, i.e., the awareness of the direction toward the readjusting of the suspended action. But there are diverse ways to be disadapted: one can be disadapted with regard to time, to space, to cause, to end, to number, etc. We see here the biological origin of the "categories" of the logicism. These categories have as their origin the questions when? where? how? why? how many? etc. And these questions again are but these diverse sorts of disadaptation becoming conscious.

The Law of Becoming Conscious also enables one to understand the nature of the sensation of the needs, such as thirst, hunger, sleep. The sensation of need is not the awareness of an objective need of the organism, for, when this need can be satisfied easily, the need is not felt. The consciousness of need only comes on *when it cannot* immediately be satisfied. This consciousness results from the becoming conscious of the reactions which tend to satisfy the need, or to compensate its bad effects.[17]

In 1901, Flournoy and I had founded the *Archives de Psychologie,* and the editing of this periodical, with its bibliographical section, proved to be, for both of us, more work than we had supposed. Besides, my energy continued to be dissipated, my interests being divided, as I have said, between psychotherapy, work in the schools, and psychology—not to mention occupations other than scientific, which are very numerous in a town like Geneva, where the civic sense is highly developed.

I had been interested in politics from very early days. When I became an elector, at the age of 20, there were two antagonistic political parties in Geneva. It was not hard to see that both of them put their own interests too often before those of the community. My youthful conscience revolted at this. I was therefore very relieved when a new party was created, thanks to proportional representation, which was introduced in Geneva, in 1892. This party, the *"Groupe national,"* which, moreover, refused to be called a "party," had as its aim the objective study of all questions, without any political bias. It had admirable principles and should have been enthusiastically welcomed by all honest citizens, but, on the contrary, it was violently attacked and ridiculed by the two main parties, which, for once, found themselves in agreement. None the less, I remained attached to this National Group and to its paper, the *"Signal de Genève."* I cooperated in editing it until 1912, when the National Group disappeared from the political stage, under the concerted attacks of its adversaries of the right and left wings. During those 20 years, I was able to see what a powerful hold the logic of feeling has, even on intelligent and honest men, and how uncommon a virtue is impartiality.

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[17] "Le sommeil et la veille." *J. de psychol.,* July, 1929, and in *Traité de Psychologie,* edited by Dumas, 2nd ed., 1929.

As a matter of fact, I have always been surprised that it should be so uncommon, for of all the virtues, it seems to me the easiest to possess. It comes so naturally to me, it seems to me to harmonize so completely with the true interest of all who wish to understand life and avoid error, that I find it difficult to regard it as an extraordinary quality; it appears to me to be the very condition of the most elementary loyalty, in daily life as well as in science. This independence of mind, this aptitude for not letting one's self be influenced by the opinion of those around one, is a characteristic trait of my family (my two brothers also belonged to the National Group, as well as Flournoy). I must hasten to add that I cannot take any credit for having shown independence of mind, for my situation has always been such as to allow me to avoid flattery.

So I have always appreciated national or international politics without preconceived ideas, according to the method which I believe should be that of the true *"Liberalism,"* i.e., of judging trees by their fruit.

I feel deep sympathy for many of the aspirations of Socialism. I hope, as it does, for a thorough reform of our iniquitous social system. I suffer all the more from the injustice it covers, in that I personally profit by it. But I cannot abide the dictatorial and dogmatic aspects of Socialism. The unlimited powers of control it ascribes to the State tends to produce a form of government which completely disregards the psychological realities of the modern man, as it expects him constantly to work apart from his personal interests. It can but lead to favoritism or red tape. On the other hand, the Socialist leaders who foster class-hatred, and use Coué's formula—only they invert it, constantly repeating to the working-man: "Every day, in every way, you are being increasingly exploited,"—these leaders stir up the discontent of the working-man and therefore diminish his capacity for happiness. As a matter of fact, I do not believe the working-man is much happier psychologically today than he used to be, though his material conditions of life have much improved since the coming of Socialism. It seems to me that if the Socialist leaders were really the friends of the working-man, they would make him realize his relative happiness, rather than cultivate in him feelings of inferiority which embitter him and gratuitiously darken his life.

What we call "Liberalism" seems to me, therefore, less a political doctrine than a method, the main characteristic of which should be freedom from preconceived ideas, i.e., a method of intellectual and moral loyalty. This means that it must rest ultimately on experience, which must be our supreme teacher in this world. And in my mind I unite Liberalism, Pragmatism, and Protestantism, which are to politics, philosophy, and religion what the experimental method is to science, a method of truth, substituting the free study of facts for the coercion of dogma or the dead-weight of prejudice.

I am extremely attached to Protestantism, thanks to which this method of free enquiry was introduced into the world and the principle of toleration into religious matters. What seems to me most monstrous, most degrading for the dignity of human personality, is that certain religions should oblige their followers to believe in spite of themselves, or rather, to declare that they believe; and that intelligent and honest men should consent to renounce what they personally consider true and just, in order to obey their Church's injunctions. I can only conceive of religious belief as the outcome of certain moral and emotional experiences which the individual feels called upon to interpret or explain, and not as a number of a priori affirmations, given out under the authority of the infallibility of a text or of a person. (For, if we adopt this latter conception, who would vouch for the presumed infallibility? The only way to guarantee it would be another infallibility, and so on, *ad infinitum*).

Moreover, Flournoy's teaching was inspired by these liberal ideas, and I doubtless owe him much in this respect. His teaching was very remarkable, he studied more particularly those semi-obscure problems which "official science" as he used to say, disdains: genius, religious psychology, and, especially, subconscious phenomena. But he came less and less often to the Laboratory and finally handed over to me its direction, in 1904. William James, who had freed himself from his Laboratory and entrusted it to Münsterberg, had been for a long time past urging his friend Flournoy to follow his example, and Flournoy yielded, in the end, to this tempting suggestion.[18]

At this time, I was engaged in experiments, at the Laboratory, on the illusions of weight, monocular stereoscopy, mental activity during hypnosis, the psycho-galvanic reflex, etc.[18a] But the question which then interested me most was that of testimony, raised by Binet and Stern. I was delighted with the idea of an applied psychology. On the one hand, it corresponded to a very definite wish of mine, to improve the conditions of daily life, from the moral and social points of view especially, and to fight against all possible errors. On the other hand, it seemed to me that applied psychology could be of the greatest use to theoretical psychology, in giving it concrete problems to solve and also in compelling it to state its results with ever greater precision and avoid being satisfied with superficial phrase-mongering, the value of the theory being measured by the success of its application.

I undertook a few group experiments on testimony, one of which gave rise to some comment: I questioned my students as to the existence or non-existence of a certain window in the hall of the University, an inner window in front of which they passed every day. Forty-one out of 54 students denied its existence. Another day, I arranged for a student to disguise himself and burst into the lecture-room, where he made a lot of noise and I sent him out. The descriptions and reports of this incident were also very poor. These experiments showed that one can have *agreement in error*. They also proved that witness is given "along the lines of probability" when no interest is present to fix memory.[19]

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[18] See *The Letters of William James,* Boston, 1920, Vol. II, p. 53; and *Arch. de psychol.,* **18**, p. 71.

[18a] All of these experiments were published in *Arch. de psychol.* The "Recherches expérimentales sur quelques processus psychiques dans un cas d'hypnose (*Arch. de psychol.,* 1909, **7**) were made in collaboration with my assistant, W. Baade, a former pupil of Professor G. E. Müller, who unfortunately died some years ago.

The problem of the size of the moon when it is on the horizon occupied me for some time, for I had noticed that the classical explanation, accepted from Ptolemy to Descartes and from Malebranche to Helmholtz, according to which the moon seems larger when it is on the horizon because it then seems further away, is refuted by observation. Inquiry showed me that everybody thinks the moon seems on the contrary *nearer* when on the horizon. I then tried to explain the illusion by the factor of interest: the moon being in the terrestrial zone is of greater importance for us and therefore seems larger. But I am quite conscious of the insufficiency of this explanation.[20]

If I spent my evenings contemplating the heavens, during the day I often went to the Lunatic Asylum, where there was at that time a most interesting case of Korsakoff's amnesia. With this patient I made many experiments on memory which I have never published in detail. I do not very well know why, as I brought to light a fact which seems to me important in its bearings upon the theory of recognition and also upon that of voluntary recollection, namely, that the contents of memory can remain unrecognized, even when they are capable of being reproduced or of starting adapted reactions.[21]

In 1901 I had had a son, and when he was four years old I noted the ease with which he found, in a music book, the page corresponding to each piece. He was naturally unable to help himself by reading, but he knew the general aspect of each page. I have proposed to call *syncretical perception* this general view of things without any discriminating of details. A baby girl born some years later helped me to demonstrate still more clearly the reality of this process. When she was only two years old and did not know a single letter of the alphabet, I easily taught her to read more than a hundred words and short sentences. I often took her to my very much amused students; but I have never published these experiments.

My theory of sleep had naturally led me to be interested in hypnosis, which I had, moreover, often used successfully with my patients. During three years, I had a fine female synocephalus (baboon), which was very lively and active but which fell into catalepsy as soon as one made passes over its head and body. It then presented the *flexibilitas cerea* of Charcot, remaining like a statue in all the attitudes, even the most uncomfortable, that one made its limbs assume. Unluckily this animal was killed by a dog before I was able to have these interesting phenomena filmed. I have had the opportunity of putting to sleep goats and pigs, and later also sheep, in like manner, by making passes over them. These animals lived with me in the mountain chalet where I was mobilized with my battalion in August, 1914. But I never had time, or knew how to *make* time, to experiment systematically![22]

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[19] "Expériences collectives sur le témoignage." *Arch. de psychol.*, 1906, **5.** See also the experiments which Marie Borst carried out in my laboratory on "L'éducabilité et la fidélité du témoignage." *Arch. de psychol.*, 1904, **3.**

[20] "L'agrandissement et la proximité apparents de la lune à l'horizon." *Arch. de psychol.*, 1905, **5.**

[21] "Recognition et moiïté." *Arch. de psychol.*, 1911, **11.** See also my memorandum in the *Rev. méd. de la suisse romande,* 1907, April.

Because of my interest in animal psychology, to which I was always sorry I could not give more time, the horses of Elberfeld and the dog of Mannheim at once attracted my attention. When Krall's well-known book was published, it was greeted by a general outcry on the part of biologists and psychologists, who passed sentence at once, at a distance, and without having seen anything themselves, of the facts reported. This attitude seemed to me full of that dogmatism which is entirely opposed to the true spirit of science. Though I was rather skeptical myself as to the reality of the feats attributed to the "performing animals," I went to Elberfeld and later to Mannheim, to examine the animals *de visu* and, if possible, by experiment. I have described these visits in detail.[23] I was not able to come to any definite conclusion, for, though I found it impossible to admit that these animals could really calculate and spell, yet I must confess that I can find no explanation for certain answers, given under conditions which seemed to preclude any possible charlatanism.

Meanwhile my interest in the applications of psychology inclined me ever more toward educational psychology.[24] Every day, in my conversations with my patients, I could see the influence of education—and more especially of errors of education—on the development of personality. Surely it is better, I thought, to try and prevent these errors, than to labor at correcting their consequences. Freud's doctrines, which were just then beginning to be widely diffused, and which Flournoy and I had received with great sympathy, though without any exaggerated enthusiasm, confirmed me in my conviction of the importance of the years of childhood for the subsequent destiny of the individual.

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[22] *Arch. des sci. phys. et nat.*, Geneva, 1911 and 1915.

[23] "Les chevaux savants d'Elberfeld." *Arch. de psychol.*, 1912, **12**; 1913, **13**. "A propos du chien de Mannheim." (With J. Larguier des Bancels.) *Arch. de psychol.*, 1913, **13**.

[24] Industrial psychology, which was then at its beginnings in the United States, also attracted me. In the spring of 1914 I had taken as the subject of one of my university courses of lectures the following subject: The technique and organization of industrial work, Taylorism, professional aptitudes, the psychology of advertising, etc.

The somewhat bitter memory of the time I had lost, during my school years, thanks to antiquated methods of teaching, had something to do, perhaps, with the direction my preoccupations were taking. Moreover, I had a son, and it was not without anxiety that I thought of him passing through the same scholastic mill, which crushes the best years of youth.

Education, exactly like medical science, is a technique which can be founded only on knowledge, and this can be given only by observation and experiment. But the psychologist is not well placed to build up, by himself, this science of the child which is so necessary to pedagogy, for he has not at his disposal the children he needs. Therefore, school teachers should be prepared to gather the materials necessary for genetic psychology.

With this thought in my mind, I organized in 1906 in my Laboratory a Seminary of Pedagogical Psychology, where future educators could be initiated into the methods of experimental pedagogy and child psychology. Geneva has no training college. The preparation of teachers consists in special lectures and practical work in classes. I hoped my Seminary would be put on the curriculum of these lectures.

Indeed, this was the case during one year, but very soon opposition was organized against it. The Professor of Pedagogy at the Faculty of Letters maintained that I was trespassing in his domain, and a new Head of the Education Department, a narrow and dogmatic politician, refused to send his future functionaries to the University, for fear they should imbibe there too liberal and independent ideas. So my Seminary died a natural death!

But I did not give up hope and I decided to found a special institute for psychology applied to education, apart from the State and University, as neither the one nor the other would have anything to do with it. Just at that time, pedology was attracting people's attention and interest in Geneva. Ad. Ferrière was championing the "New School" movement; Aug. Lemaitre was studying adolescents; Alice Descoeudres was verifying Binet's tests; and the Misses

Audemars and Lafendel were trying to introduce reforms in the kindergartens, where they taught.

The welcome accorded to my book, *Psychologie de l'enfant,* four editions of which had been called for in rapid succession, and which was soon translated into six or seven languages, and the good fortune I had to obtain the collaboration of Pierre Bovet, then Professor at Neuchâtel University, gave me such encouragement that, in October, 1912, I was able to open a school for the sciences of education, with the help of a group of friends who backed me financially.

I called this school the *J. J. Rousseau Institute,* which was very natural: the bicentenary of the birth of "Geneva's famous citizen" had been celebrated that same year, and is it not in his *"Emile"* that the necessity of the teacher's "studying his pupils" is affirmed for the first time? I have shown, I believe, that the basic principles of the science of the child are to be found already in that great book.[25]

The motto of our Institute, *Discat a puero magister,* indicates its inspiration: we aim at placing the future educators as much as is possible in contact with children, so that they should get to know them—and to love them. And the spirit which inspires our Institute is the scientific spirit, i.e., freedom from preconceived opinions and the constant care to "try everything and keep that which is good," according to the apostle's liberal words. In pedagogy, as in science, it is experience which shows whether theories and methods rest on a correct basis.[26]

It had, however, appeared to us from the start that child psychology warranted certain inferences for the practice of education. We professed a *functional conception of education,* which we did not bring forward as a dogma, but as the interpretation that agreed most closely with all we know of the laws of human conduct.[27]

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[25] "J. J. Rousseau et la signification de l'enfance." *Rev. de mét. et de mor.,* 1912, May.

[26] On the creation and development of the Rousseau Institute, see "Un institut des sciences de l'éducation et les besoins auxquels il répond." *Arch. de psychol.,* 1912, **12**. "The J. J. Rousseau Institute." *Ped. Sem.,* 1925, **22**. P. Bovet, "Un institut de pédagogie expérimentalle." *Année psychol.,* 1912, **18**. "L'Institut J. J. Rousseau de 1912 à 1917." *Arch. de psychol.,* 1917, **16**.

[27] "La conception fonctionnelle de l'éducation." *Bull. Soc. psychol. de l'enfant* (Paris), 1911, Nov., p. 45. "Rapport au Congrès d'Hygiène mentale de Paris, juin, 1922." *Informateur des aliénistes,* 1922, Dec. "La psychologie de l'école active." *Interméd. des Educateurs,* 1923, Dec. "Réflexions d'un psychologue, la pensée et le savoir." *Annuaire de l'instruction pub.,* Lausanne, 1925.

According to this functional conception, mental processes are considered as functions which enter spontaneously into play when certain needs are present. Therefore, if the child is placed in circumstances calculated to awaken these needs, these desires, he will be active. I wish to mention here what an encouragement it has been for me to find developed in John Dewey's remarkable books this functional conception, which I had reached myself through my biological interpretation of mental activity.[28] I had also been much helped by Kerschensteiner's studies—in 1911, I had visited his *Arbeitschulen*, in Munich—and the school-experiments of my old friend Decroly.

Under P. Bovet's exceedingly competent direction, the Rousseau Institute soon attracted pupils from all over the world. From the very start, all my preoccupations had centered around it. It was a constructive work, and at the same time active propaganda for the methods which were dear to me, and this stirred up my enthusiasm. I devoted myself completely to it, to the prejudice, I must admit, of my scientific work. I have never known how to reserve complete days for my personal work, apart from the claims of my teaching and the lectures I am constantly asked to give everywhere, in Paris or London, Madrid or the Hague, Vienna or Brussels.

Our Institute encountered material difficulties and also the latent opposition of the school authorities. Today, the situation is easier: the Institute's utility has been recognized by the State of Geneva, by which it is subsidized, and which has recently incorporated it in the University (as I had asked 20 years ago). The Laura Spelman Rockefeller Foundation has also helped us financially and allowed us thus to develop a number of our sections.

To give an exact picture of the J. J. Rousseau Institute, I should speak of the friendly relations which unite teachers and pupils and make of them one big family. I should also describe the fine work done by Misses Descoeudres, Audemars, and Lafendel, Headmistresses of the *Maison des Petits,* by Professors Bovet, Ferrière, Piaget, Walther, Mme. Antipoff, etc. But alas! I have received the deplorable instructions to speak only of myself here.

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[28] At my request, L. S. Pidoux translated into French four studies of John Dewey, which were published under the title, *L'école de l'enfant, Collection d'actualités pédagogiques de l'Institut Rousseau,* Neuchâtel and Paris, 1913. (2nd ed.) 1922. I added an introduction to this volume on "John Dewey's Pedagogy," which I wrote *con amore,* for I have the greatest admiration for the depth and distinction of thought of this great thinker. He was good enough to write to me that my introduction "is most sympathetic and generous, traits that combined with his exactness of comprehension and lucidity of statement, give all and more than all that the most exacting author could wish for in the way of an introduction to a foreign public. It is impossible to imagine such a task performed more delightfully." Needless to say, this very kind appreciation gave me the greatest pleasure.

In 1918 the Rousseau Institute started a vocational guidance office, which opened up new fields of action, and was the occasion of various experimental studies.[29]

Before leaving the subject of education, I must mention the invitation I received, in 1928, from the Egyptian Government to report on the reorganization of the schools and training colleges of Egypt. I, therefore, spent the winter 1928-1929 in Cairo. With the help of Miss Bieneman, a former pupil of the Rousseau Institute, an extensive inquiry was carried out by means of various tests on the mental development of the Egyptian child. The obligation we were under, to reach practical solutions, showed me more clearly than ever the necessity for a solidly established science of the child and for experimental pedagogy. And I cannot understand why most practicing educationalists, far from eagerly asking for this science, which would be of daily value to them, attack those who attempt to found it!

In 1908 I was appointed Associate Professor of Psychology. In 1915 I succeeded Flournoy, who was elected Professor of the Philosophy of Science. In my teaching, I have aimed especially at making my students wish to observe and experiment themselves, placing before them problems to be solved and insisting upon methods of investigation and the causes of error which we meet with at every step. My lectures have always been more or less improvised. I used to feel it my duty to keep in touch with the progress of psychology in all countries, and perused the numerous books and periodicals which accumulated, each week, on the shelves of my bookcases. Today, psychological publications (without mentioning those of connected sciences, like psychiatry, physiology, etc.) have increased in such proportions that this becomes materially impossible.

I find it as easy to speak, to give a lecture without any preparation, on a question which is familiar to me, of course, as I find it hard to write. I think I have discovered the reason for this. When I am writing, I am torn between two tendencies of my nature: on the one hand, I would like to converse freely, for I hate that dull and pedantic dryness of tone which characterizes so many scientific writings. On the other hand, I am compelled, by my desire for logical precision, to connect the various parts of my work in a rational order. But this is often impossible, for the paper he writes on only provides the author with a surface of two dimensions (at most, three, if we count the notes at the bottom of the page!) and yet the ramifications of thought run in numerous directions, which ought all to be followed simultaneously—for they implicate each other—if the subject is to be successfully treated. These difficulties of composition discourage me so, that I abandon the whole undertaking in despair. The reason why I never wrote the book on *Intelligence,* which was to be the second volume of my *Psychologie de l'enfant,* is perhaps because I never managed to draw up the index to my logical satisfaction!

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[29] See those of Fontégne and Solari, Bieneman, Walther, Ehinger, etc. See also my publication "L'orientation professionnelle, ses problèmes et ses méthodes," edited in 1922 by the International Labour Office (English, Spanish, Polish, Rumanian, and Russian translations).

And yet I am quite conscious of the absurdity of these scruples which are not excused by my desire for precision. But they are mightier than I am. . . .

It is an interesting point for psychology, this division of one's aspirations in two contradictory groups, which run counter to each other and cause continual inhibitions. And all my scientific activity is dominated by this conflict between these two contrary attitudes, which might be called, according to Oswald's terminology, the *romantic* and the *classic* attitudes. My wish was to be an observer, an explorer, an experimenter, a discoverer. But I have been more especially a systematizer, a teacher, an organizer of already acquired knowledge, a composer of "general reviews" to "bring a question up to date."[29a] My book on child psychology is full of divisions, subdivisions, and pedantic classifications which set all my romantic self on edge . . . . and I suffer from it all the more because this last self seems to me to correspond to my "real self," whilst the classic tendency is rather to be compared to an outsider, a fiend which seizes me by the throat and brutally imposes its will on mine. This comedy, which sometimes turns to tragedy, might be entitled "the Classic in spite of himself!"

My "real self," however, does not think all classification useless. Classification is often necessary in teaching to help students to set their ideas in order. It is with this aim in view that I have proposed a classification of psychological methods, and another of the methods of animal psychology, which may, I believe, be of use to the experimenter, showing him at a glance all the instruments he disposes of.[30] This same desire for clearness of thought led me to study terminology, and I would have liked our Congresses to be a means of attaining unity in this respect, as is the case with those of chemists and botanists.[31] This is the reason which made Binet say I had a taste for doing the "police work" of psychology.[32] A taste, no, but I considered it a necessity, and I must admit, it satisfies at the same time my systematizing demon and, perhaps, the sublimated remnants of my infantile desire for domination!

[29a] See, for example, "Revue générale sur l'agnosie." *Année psychol.,* 1900, **6**; "La faculté d'orientation lointaine," *Arch. de psychol.,* 1903, **2**; "La psychologie judiciare," *Année psychol.,* 1906, **12**.

Is it the prospect of being able to indulge this desire which made me accept with joy the post of General Secretary of the Second International Congress of Philosophy held in Geneva in 1904, and later of the Sixth International Congress of Psychology, presided over by Flournoy, in 1909? Anyhow, I tried to stem the flood of reports, which usually encroaches upon the time which ought to be given to discussion and thus chokes the true life of a congress. I also put on the agenda of these two meetings questions of the day, with reports printed beforehand. In 1909 we had the good fortune to have these reports drawn up by such men as Dessoir, Morton Prince, Höffding, Leuba, Külpe, Sollier, Loeb, Baldwin, W. Nagel, etc., who were all present at the Congress.

I have been to many Congresses and have always found it a real pleasure to get acquainted there with those I consider as my masters, or with the colleagues whose works I read and of whom many have become my very good friends. I must confess that this personal side of the Congresses has always meant more to me than the "official" side, that of reports and communications. And I would willingly subscribe to the wish which Flournoy once expressed, for a "Congress without reports!" Are not bonds of friendship between men of all nations the indispensable affective substructure of the League of Nations' work?

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[30] "Classification et plan des méthodes psychologiques." *Arch. de psychol.*, 1908, **7**. "Die Methoden der tierpsychologischen Beobachtungen und Versuche." *Ber. d. III. Kong. f. exper. Psychol.*, 1908.

[31] "Sur la définition de la perception." *C. r. Cong. int. de psychol.*, Paris, 1900. "Rapport sur la terminologie psychologique." *C. r. Cong. int. de psychol.*, Geneva, 1909. Collaboration au *Vocabulaire de la philosophie*, de Lalande.

[32] Binet, *Année psychol.*, 1911, **17**, p. 490.

Many events ought still to be mentioned: the War, which, even to a "neutral," was a long, anxious period; Flournoy's death in 1920, which seemed to us like the disappearance of a great light which has so often showed us our way;[33] the establishment at Geneva of the League of Nations, which turned our thoughts more strongly than ever toward international politics and which has been at the same time an enrichment and an obstacle in the concentration which is indispensable to scientific work; the creation of the International Bureau of Education. . . .

But the space still at my disposal must be employed in answering a few questions, at the request of the editors of this volume. And first of all, what do I myself think of my contribution to psychology?

Well, it seems to me very meager, compared to my earlier plans and hopes! If I have been of use to psychology, it must be for having continually endeavored to give it an empirical, and more especially a *biological* basis, and diverting it from metaphysical, a priori and dogmatic conceptions which limit its field of action. It seems to me that the *functional conception* I advocate throws new light on the problem of sleep and hysteria, and on that of intelligence and of will; and that my *Law of Momentary Interest* does indeed express the procedure of all mental activity and of all conduct.

My *Law of Becoming Conscious* can be useful, I think, for genetic psychology. I feel I have been of service to pedagogy in endeavoring to give it a *scientific basis,* and also in fighting for *play* to be given its proper place, not only in education, but even in teaching.

Second question: What do I consider are the most important problems of modern psychology? And in what measure have I contributed to their solution? I believe these problems are as follows:

1) The problem of the adaptation of conduct, the direction of thought, creative imagination, etc.

2) The problem of character and of the relations between organic constitution and character or mental aptitudes.

3) The problem of heredity and environment, or of constitution and education: in what measure can acquired experience or education modify an individual's constitution, or, at any rate, the natural reactions dictated by this constitution?

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[33] As I could not speak here of Flournoy as much as I wished to, I refer to my paper, "Th. Flournoy, sa vie et son œuvre." *Arch. de psychol.,* 1921, **18.**

4) The problem of the connection between affectivity and intelligence, which includes the study of the action of the subconscious. This is the problem of that profound drama of human activity, where the egoistic desires and ambitions of the Self struggle against the imperious claims of Reality, or, to borrow Freud's language, the principle of pleasure struggles against that of reality. But the matter is further complicated by the fact that the individual has to adapt himself to two realities: the *physical* reality, which requires the pursuit of truth, and the *social* reality, which calls upon either rational or affective thought, or suggestion, or even falsehood.

5) The problem, closely related to the preceding, of the Self, of the Will (and of *Aboulia*), and of the Ideal. If the Ideal which guides the aspirations of our higher Self, in the conflict of the Will, is itself but the expression of our deepest tendencies, why should the pursuit of this ideal be so difficult, why should it fail so often because of the interference of the lower Self? It is no solution to say that the Ideal is given by Society and not by the aspirations of the Self, for all the members of any given Society do not accept its ideal: we only adopt an ideal suggested by other people or by Society if it corresponds to our inmost aspirations.

It often happens also that there is no connection between conduct and inner conviction, for instance, So and So, who behaves optimistically, may be, at the bottom on his heart, a confirmed pessimist. (I am somewhat in that case, at least, I find in myself optimistic and pessimistic tendencies coexisting, and it is practically impossible for me to say which corresponds to my "real Self." It varies according to the weather, according to what I have eaten or drunk!) Ah! What a problem, that of the "real Self," even when considered strictly from the point of view of empiricism, which is the only one I here take. And under this—or *these*—empiric Self or Selves, I discover in myself yet another deeper Self, the "I who judges," while he himself remains neutral, impartial, free from affectivity.[34]

Third question: What have I contributed to the solution of these problems? Concerning Number 1, twelve years ago I undertook a series of experiments by an introspective method which I have called the *method of spoken reflection.* The subject is given a problem to solve, e.g., a conundrum, a puzzle, or a story in pictures, of which only the first and last are shown and the others have to be found. He is asked *to think aloud* as he works at the solution of the problem. I hope to be able to finish this study, of which one half has already been written many years ago.

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[34] "Note sur la localisation du moi." *Arch. de psychol.,* 1924, **19**, p. 182 (en note).

To examine thoroughly Problems 2 and 3, psychological tests are necessary. But the method of tests itself raises many problems. I have studied some of these, especially the problem of the *constancy* of individuals. The aptitude of a subject often varies from one day to another. What is the "real measure" of the aptitude of an individual? My assistants, Mmes. Feygin and Antipoff, and I have done some research on this important question.[35]

Another closely connected problem, which is of great importance for vocational guidance, is that of the possibility of modifying original aptitudes by practice. Can a certain individual, whose initial output is superior, be surpassed, after a time of practice, by others, whose initial output was inferior? This seems to me the most important question in vocational selection. I had asked that it be put on the program of the First International Conference of Psychotechnics, held in Geneva in 1920; I raised it again at the following Conferences, at Barcelona, in 1921, in Paris, in 1927, and we are now working at collecting data on the subject.[36]

Problem Number 4 is one of those which preoccupy me most, from the point of view of its importance for daily life, for the formation of opinions, prejudices, and political, religious, national or other preconceived ideas. I consider that, in schools, every child's attention should be drawn to the deformations of straightforward and loyal thought which are due to affectivity. I have indeed only once attempted to examine this difficult question of affective thought, and have rather shown the complication of this group of phenomena, than thrown any light up it.[37]

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[35] "De la constance des sujets à l'égard des tests d'aptitude." *Arch. de psychol.*, 1919, **17.** "Sur la constance des épreuves dynamométriques." *Festschr. zu H. Griesbach*, Giessen, 1925. Léa Feygin, "Expériences sur la constance des temps de réaction." *Arch. de psychol.*, 1925, **19.** H. Antipoff, "Contribution à l'étude de la constance des sujets." *Arch. de psychol.*, 1927, **23**; and "L'évolution et la variabilité des fonctions psychomotrices." *Arch. de psychol.*, 1928, **21.** Cf. also *Comment diagnostiquer les aptitudes chez les écoliers?* Paris, 1924.

[36] I have also proposed criteria for distinguishing the age tests from the ability tests; "Tests de développement et tests d'aptitude." *Arch. de psychol.*, 1914, **14.**

[37] "L'auto-justification." *Arch. de psychol.*, 1927, **20.**

As to Problem 5, I had begun to state its elements in a book on *The Will,* the unfinished manuscript of which has been lying for the last seven years at the bottom of some drawer.

Fourth question: What, in all probability, will be the development of psychology in the next generation?

It is difficult to say, for the coming of one man of genius is enough to change the whole aspect of a science. Will psychology have its Einstein? Anyhow, it has had Binet and Freud. Who, indeed, round about 1890, could have predicted the direction of its present development?

But, though I do not feel capable of describing the psychology of tomorrow, I would like to suggest what I consider it ought to do in order to progress.

First of all, free itself from all dogmatism, such as associationism, reflexology, behaviorism,[38] "verstehende" or "erklärende Psychologie," and depend solely on observation and experimentation. What is the use of wantonly limiting the scope of psychology, prescribing beforehand the concepts which will be of value to it? I believe one should be eclectic and adopt provisionally all those points of view which would appear to be of practical value, even if they be contradictory—that of mechanism or of behaviorism (whenever possible) as well as that of functionalism or of personalism. Their value will be measured by their respective fecundity, and those that are beaten in this struggle for existence will disappear of themselves. But, I ask you, is there any sense in determining in advance those that *must* be useful, and those which are not allowed to be useful! It is not *before* building a science that one should pass a decree, dogmatically, *a priori,* on the principles and concepts accepted as useful building materials! On the contrary, it is *after* the construction is finished that one can say empirically, *a posteriori,* which were the ones which proved necessary and therefore legitimate. The psychologist must be a man of science, not a metaphysician. His attitude must be pragmatic, not dogmatic. With unprejudiced mind, he must make use of every concept that can help him, and yet always be ready to abandon it as soon as it is no longer of service.

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[38] I do not oppose the study of behavior, but simply behaviorism as a dogmatism. In 1912, I wrote, "The problem of psychology is the problem of behavior." ("Point de vue physico-chimique et point de vue psychologique," *Scientia,* 1912, p. 258. I have not changed my point of view. However, there are certain precautions to be taken; behavior cannot be defined; one cannot distinguish it from other processes of the organism without calling up notions of purpose, plan, internal preparations, etc., i.e., mental activity.

This subjection to fact in no way means that the psychologist must despise hypothesis and theory. Their part, on the contrary, is an immensely useful one, for they suggest experiments. But he must never lose sight of their conjectural nature: as long as they have not been verified, they function only as provisional truths, if one may so speak.

Finally, the last question: If I suddenly became a young psychologist again, which is the problem which would most inspire me and which I would begin to work on? I think I would be most attracted by the problems indicated under Numbers 2 and 3, because of their great practical importance. In that case, it would certainly be wise to use the time I would then have at my disposal in undertaking observations on a certain number of children, their character, aptitudes, the influence of their environment, etc., which could be continued during their lives.

I have come to the end of my reminiscences. What interest they can have for anyone, I hardly know—save for myself, to whom they have shown how difficult it is, even for a psychologist, to have a clear vision of one's self!

RAYMOND DODGE

It would be misleading to label this an autobiography in the usual sense of the term. It is not a chronicle of personal contacts with people or events. They have been too restricted to be especially interesting. It would be superfluous to rewrite an account of my scientific work. What I have accomplished is documented in formal reports. There are, however, certain aspects of scientific work that usually find no formal expression, such as personal background and bias, the methods of constructive thinking, and the appraisal of problems. Moreover, there are occasional moments of insight when the systematic implications of one's work seem to become clear. These the experimentalist commonly labels premature and suppresses in the pursuit of more data. It is a reasonable hope that in a lifetime of devotion to experimental science certain of these less formal aspects are worth writing out. It would be something of a pity if I had not seen further than I could go and if the occasional moments of insight should entirely fail of expression.

The Personal Equation

Intellectual inheritance is not only beyond our control but also largely beyond our present powers of analysis. At any rate, I have little positive knowledge about my own. I seem to have had an endowment defective in auditory and somewhat above the average in motor and kinaesthetic imagery. Perhaps in consequence of this endowment linguistic pursuits, including public speaking, are relatively difficult, while mechanical invention and the manipulation of instruments are pleasant and relatively successful. It was Erdmann's judgment that I ought to train for engineering and he backed that judgment by an offer to advance funds for a technical course with the expressed belief that I would soon be able to finance my own psychological experimentation. He may have been right.

I believe my father was an unusually able man. At any rate he was always studying something that seemed worth while. An apothecary by trade, he took an M.D. at Harvard in middle life and was an acceptable preacher in small parishes in later years. My mother who was entirely non-musical had an active imagination and ability to plan. Both were deeply religious in what was then a very liberal and advanced way.

I was born February 20, 1871. My early memories are largely connected with the drug store, where I had growing responsibilities; with my father's little office containing his library of medical, philosophical, and religious books, which intrigued me even when I was too young to understand them; and especially with the workshop in the rear, where I had access to good tools and was usually constructing something. Those play products of the little shop were probably the intellectual antecedents of registering devices, tachistoscopes, and general instrumentation.

On our infrequent walks together my father introduced me to the beauty of nature and the world of speculation. Appreciation of the beautiful is an invaluable asset to anyone. When combined with an urge to expression, it not only enriches the content of life but may on occasion have an important mental prophylactic value. I still vividly remember my first intimation of the difficulties of the concept of space when we argued together on Powderhouse Hill as to what might be beyond the blue sky. My first conjecture of a stone wall proved untenable, but the problem fascinated me even in childhood. My first absorption in philosophical problems came in my freshman year in Williams College with the accidental possession of a volume of John Fiske's *Essays* which led me to his *Cosmic Philosophy* and to Spencer. About this time came the idea of specializing in philosophy as far as the course of study at Williams permitted. Under the stimulating influences of Professor Russell and President Carter I decided to do graduate work in that subject and tackled Kant's *Critique of Pure Reason*. It fascinated me as a student, and has remained an important background of my thinking. Its influence is readily traceable together with the apperception theory of Erdmann in my "Working Hypothesis for Inner Psychophysics."

My early economic resources were very limited. Home finances forced me to earn my own way at college from sophomore year on. I had the good fortune to obtain employment in the college library under the guidance of my constant friend, Professor Burr. That intimate contact with books was worth fully as much as the financial help. Appointment as assistant librarian provided a year of part-time graduate study and a savings account of $500, enough, I decided, to start work for the doctorate. That sum actually financed me through two years of study in Germany. True, I travelled steerage. My room was small and often cold; food limited in cost to one mark a day was often scanty and sometimes ill adapted to my digestive system. But the game was interesting, and I enjoyed some luxuries. I went to the theater once a week, bought some invaluable second-hand books, and had the stimulating society of splendid friends. Still I doubt if I could recommend another to go through what I did. But the experience has made me very sympathetic with impecunious students. Unfortunately the long training in poverty left many scars, scientific as well as social. Even now I spend considerable time trying to do things in the laboratory in the simplest and least expensive way, and am oppressed and inhibited by expensive construction.

My decision to go to the University of Halle was determined by a misfortune, a conviction, and an accident. The misfortune seems instructive. During the year of graduate study at Williams I prepared a thesis on certain differences between psychological and philosophical conceptions of space, which I presented with applications for scholarship aid at both Harvard and Columbia. It was one of the few great disappointments of my life when I was refused at both institutions. That experience may be the background of my distrust of predictive tests. The conviction that influenced me was that if I was to become a philosopher I must know the German language, and if I was ever to learn the German language with my linguistic handicap I must learn it where it was spoken. The argument was probably sound. It was an accident that the copy of Kant's *Kritik der reinen Vernunft* which was given to me by Professor Russell was edited by Benno Erdmann. After my disappointment, the conviction and the accident led me to Halle-an-der-Saale.

I am aware of three maxims that seem to have determined my reactions at important crises. One came from my father. On the eve of my departure to make my own way in college at the beginning of my Sophomore year he urged me in a good-bye talk to endeavor wherever I was to make myself indispensable. It was an impressive farewell, and the maxim determined my conduct in many situations in the Williams Library, in Halle, and in later life. It probably expresses a dominant epicritic system in my scientific and practical behavior. The second maxim came from Professor Mears of Williams. After two hours of fruitless and discouraging effort to discover the nature of a given chemical solution, he laid his hand on my shoulder and said with an earnestness that made it indelible, "Dodge, there is a cause for everything, even our failures." The phrase has come to me again and again in periods of discouragement, in scientific cogitation, and in planning apparatus. One may doubt its philosophical adequacy, but there is no doubt about its practical helpfulness. The last maxim is less clearly formulated but consists in a profound distrust of traditional impossibility. I cannot trace its origin as I can that of the other two, but the fact remains that to have someone tell me a thing is impossible is very stimulating. It began to influence my reactions quite early, certainly when as boys in high school we planned our vacation trips in our home-made canoes. In addition to these conscious principles of adjustment there is undoubtedly another, less clearly defined drive which I believe is common to almost all productive work whether in science, in art, or in industry. It may be expressed as the desire to spend one's life for something of intrinsic worth and of relative permanence.

The Transition to Psychology

My main college interest was in philosophy but at that time philosophy and psychology were inextricably intermixed. At Halle, also, my interests were about evenly divided until the coincidence of a second humiliating experience and an unprecedented bit of good fortune diverted me into experimental psychology. The humiliation occurred in connection with an essay for Erdmann's seminar on Kant's "Inner Sense." It involved a nice point of interpretation and I missed it. My kind friend's face was not altogether sad as he said, "Herr Dodge, I fear you will never make a philosopher." The good fortune is a longer story. It was during the first semester at Halle that Erdmann held a seminar on the psychology of reading, which I was permitted to attend though my linguistic difficulties made me mostly either a silent or a difficult guest. In one of the sessions of the seminar he expressed the need of more exact tachistoscopic data for an understanding of the reading process. In his opinion the only satisfactory tachistoscope would be one which permitted binocular observation, complete accommodation of the eyes, and exposed all parts of the word simultaneously. This latter desideratum seemed impossible both to Professor Erdmann and the consulting physicists. My initiation into experimental psychology may be said to have started with that technical problem. I adopted it as my own, lived with it, and gradually evolved the Erdmann-Dodge tachistoscope.

But my interest in speculative philosophy has never disappeared, and I am inclined to believe that the study of the history of philosophy and the logic of science form a valuable background for estimating the possibilities and the limitations of experimental evidence.

The Erdmann-Dodge Experiments on Reading

My engineering of apparatus has always proceeded in much the same behavior formula—a sort of mental trial and error of available expedients to meet experimental demands or desiderata, followed by rough construction and subsequent refinement where necessary. I doubt if there are any other important differences between invention and other adaptive planning except those which are involved in the nature of the situations and the kind of resources that are mobilized to meet them. However, I shall not defend that thesis here, but only recount the steps in designing my first piece of psychological apparatus as I chronicled or remember them. That period of my life was an anxious but thrilling one.

When I adopted Erdmann's problem of an instrument for the simultaneous exposure of words and letters, the use of moving screens had already been eliminated by discussion in the seminar. I canvassed every scheme I could think of. How the idea of a lens arose I do not know, but it rapidly justified itself to me, though my first effort to make it clear to Erdmann was a dismal failure. The difficulties were partly linguistic, so I made a crude model using a pasteboard box and a pocket lens that I always carried. The underlying principle was that the aperture of a lens may be increased from or decreased to zero without distorting the image. I vividly remember the demonstration of that little model. The lens shutter operated by hand. As I exposed a large letter *H* several times actually simultaneously in all its parts, Erdmann's conviction that it couldn't be done melted away, giving place to the fear that I was imposing on him some sort of American hocus-pocus. He seized the shutter himself and worked it several times before he lifted a beaming face with the remark that the thing really worked. That moment began the complete trust and beautiful friendship that lasted through everything until his untimely death after the War. It was one of the three or four great moments of my life. Plans were drawn and laid before the physicist, Professor Dorn, who after a period of rather intense criticism finally gave enthusiastic approval.

The gradual development of the tachistoscope was an absorbing episode. To justify Erdmann's boundless faith and what seemed to me the huge cost of the apparatus seemed like a terrific responsibility.

I can still feel something of the anxiety and glory of those days. An untrained novice was staking his future on a single enterprise and had resolved if it proved a failure to reimburse the University with what remained of his funds and return to a drug-store. Letters from home and from the noble girl to whom I was engaged were a great help. The halo of glory came from a rapidly increasing measure of companionship with a great scholar.

Two details of the construction gave Professor Erdmann peculiar satisfaction. The first was the rigid table I designed for the high-speed rotary shutter. The other was the auxiliary chronograph with its ink pens and paper ribbons. He was immensely pleased at the comments of the master carpenter who made the former, and helped me to publish a description of the chronograph in the *Zeitschrift*. That was my first publication. The tachistoscope was not in working condition until the beginning of the second year; then, for almost two years master and pupil worked together at the psychology of reading, either on experiments in the little Institute, or on theory in his spacious book-lined study. Those afternoons are delightful memories. During them I learned how a philosophically and logically trained mind formulated experimental hypotheses, built up experimental situations, estimated evidence, and sought just expression of results. The master's mind was always orderly and exact, the pupil's often vagrant and exploratory, impatient of experimental routine, seeking insight and crucial experiments. With growing respect and admiration for the master's intellect there developed also an affection amounting almost to adoration. His solicitude for my welfare and his later letters indicate that the affection was not one-sided. On Christmas Eves the foreigner had his place at the Professor's *Tannenbaum* with his own table of gifts, among which things to eat were thoughtfully numerous. On one occasion as the candles burned low, a twig caught fire and filled the room with the fragrance of burning spruce. I looked around a little apprehensively and found Erdmann's eyes flooded with tears. "The scent brings vivid recollections of my childhood," he explained. There followed a talk on the emotional peculiarities of the sense of smell.

On the second Easter holidays he thought that I was not looking fit and proposed a tramp in the Hartz mountains—scientific discussion strictly forbidden. He reassured me as to the probable expense, exlaining that he knew the less frequented places. They were memorable days with a charming companion, second only in retrospect to the week in later years when we wandered together in the valleys back of Mentone—with no tabu on scientific conversation.

Some humorous episodes punctuated our work together. He called to me anxiously one afternoon while he was operating the tachistoscope and I was serving as subject, complaining that the whole room was wavering. It was a hilarious moment when we discovered that the phenomenon had an objective basis, due to convection currents set up by the big kerosene lamp. Then there was the Scot who wanted to enter the experiment with us. Erdmann couldn't understand his variety of English and asked me to find out his real interest. With entire frankness the Scot explained that he had always thought there was nothing in experimental psychology but wanted to be sure. My German was an unfailing source of amusement to the younger members of the Professor's family, especially my pronunciation of *ö, ü,* and *l.*

Those who are familiar with the *Psychologische Untersuchungen über das Lesen* will remember that not all the experimental material relates to tachistoscopic observation. The first few chapters concern the discovery, measurement, and interpretation of the alternation of eye-movements and still fixations in reading. Some of these fundamental observations really came relatively late. I distinctly remember my first insight into the meaning of that alternation. We had been observing reading eyes for some time both with a mirror and with a micrometer telescope, when I tried to observe my own eye-movements. The striking and significant result was that, while the eye-movements of another were clearly visible, I could not see my own eyes move in a mirror. The experiment was singularly precise. It would be very difficult or impossible to synchronize tachistoscopic exposure with the duration of short saccadic eye-movements. A later study succeeded in doing this for extensive eye-movements only by utilizing the lateral displacement of the pupil as a shutter exposing the experimental stimuli when the pupil was exactly opposite the opening of a funnel-shaped screen. Experiments with this device rendered improbable Holt's hypothesis of a central anaesthesia during saccadic eye-movement, and disproved Cattell's hypothesis of quick perception during rapid eye-movement. Characteristically, he has been my friend ever since. The new data confirmed and extended the old; but the law that there is no effective vision of a complex visual field during refixation eye-movements was first established by trying to see my own eyes move with the aid of a mirror.

The corollary that effective vision occurs only during fixation justified static tachistoscopic exposure of letter and word groups and opened the way for the study of the perceptual importance of word form and later of phrase and page form. The consequent study of the perceptual interrelation of form and detail probably had some influence on the development of the Gestalt psychology. The importance of word form in reading has been exploited in the modern pedagogy of reading—to my mind somewhat over-exploited. The participation of peripheral or prefixational vision in the reading process was determined in a later study but has been practically ignored in the psychologies of reading. Notwithstanding the accumulation of a large amount of experimental data, insight into the relation between eye-movements and mental processes remains practically where it was left in the *Untersuchungen,* and in the "Experimental Study of Visual Fixation."

The determinants of the sharply defined fixation movements are still quite unknown. It has been rather generally assumed that the major determinant was visual, whereas it seems to me there is some evidence of a more complex neural integration involving both habit and meaning. I believe that a major contribution to the psychology of the development of meaning consciousness could be made by properly devised tachistoscopic experiments combined with recorded eye-movements and controlled pre-exposure suggestions. The general plan might well be that used in the "Experimental Study of Visual Fixation."

I have also been disappointed that pedagogy has done so little to develop a better technique for adult reading. Exploratory, unpublished experiments convince me that such applications of our knowledge are entirely practicable and that with a little patience and ingenuity both the speed of reading and the understanding of what one reads could be notably improved for the average adult.

The most important results of our observations on reading were doubtless the contribution to Erdmann's theory of apperception. At least this was most interesting to us both. It has been the background of much of my psychological thinking. More recent experiments on rectilinear and rotary oscillation indicate anew the importance of the arousal of the residua of past experiences and their fusion with the immediate consequences of stimulation. The best exposition of Erdmann's theory of apperception occurs in his last long paper, *Reproductions Psychologie,* which was written from the standpoint of our experiments on reading.

Writing the book together took the entire year after I received my degree, when I bore the title of Assistant to Professor Erdmann. Residence in Germany was made possible by a stipend from the Berlin Academy and a loan from America. Our time together was divided between the Laboratory and Erdmann's study. Though I had the privilege of writing the first draft of most of the chapters, the final form and literary style are typically Erdmann's. The monograph is the product of the happiest and most perfect collaboration I have ever known. To me it was a most important lesson in the delicate art of cooperative scientific work.

THE DOCTOR'S THESIS

It was in 1896 in the midst of our studies on reading that I took my Doctor's examination *magna cum laude,* thanks doubtless to Erdmann's persuasive ability. My thesis was another outgrowth of the seminar, where it became evident that my verbal imagery was predominantly kinaesthetic or motor. *Die motorischen Wortvorstellungen* was my first published essay in descriptive psychology. The study of the psychology of language which its preparation entailed was probably more important to me than the thesis itself. The most notable thing about its preparation was the generosity of my master. The time he spent in discussion and in coaxing my German into presentable form made a heavy debt I have tried to repay in analogous service to my various students.

TEACHING AND RESEARCH

Towards the end of my German residence came the first appointment to a teaching position—at Ursinus College, Collegeville, Pennsylvania. In addition to the burden of a heavy schedule many of my students were older than I was, and some were very sure of themselves. That year I taught Psychology, Logic, History of Philosophy, Ethics, Aesthetics, Pedagogy, and the History of English Literature. Don't laugh! It was tragedy. To fill my normal schedule of twenty-two hours a week, I was supposed to take some work in the Ursinus Academy. Fortunately, the young woman whom I had just married was prepared and willing to relieve me of that. Conditions at Ursinus have changed since then, but the crime of burying youthful research enthusiasm and scientific interest under heavy loads of teaching during the most productive years still continues in some institutions in spite of ameliorating post-doctorate fellowships. By good fortune, in my case the situation was relieved by my appointment to an instructorship in Philosophy at Wesleyan under that notable teacher and scholar, Professor A. C. Armstrong. There productive work was not only permitted but definitely encouraged. The institution owes much to the enthusiasm and example of the physiological chemist, Professor Atwater, of calorimeter fame. As an example of that influence, he stopped me one morning on the campus to inquire what I was doing. I explained that my time was pretty well occupied in developing new courses. "Naturally that takes some time," he replied, "but what research are you engaged in?" adding a judgment, which I have frequently found to be true, that if one permits his constructive work to lapse even for a single year he is in grave danger of never recovering his enthusiasm for it. I owe much to the spirit of that institution and to the friendships and asociations of the twenty-six years which were spent at Wesleyan. To particularize those obligations would make a long story.

Fortunately or unfortunately I enjoyed teaching and its interminable problems, academic and personal. It seems to me that teaching is more closely analogous to scientific investigation than many persons will allow. There has been no little controversy in well-informed circles as to the place of research in small colleges. There is no doubt that it is less economical than that conducted in research institutions. There is some danger in a division of interests and more danger that an investigator will find too little time for personal contacts with his students. Long detailed series of exact experiments are certainly difficult to combine with teaching, but I am convinced that unless a teacher is alive to the problems of his science and actively engaged in the discovery and estimation of evidence for their solution he probably falls something short of what a teacher should be.

Erdmann was ever my model. With the passing of the years his seminars stand out as supreme examples of the fine art of teaching. The method was an adaptation of the Socratic dialogue. It coincided exactly with my college dream of what I wanted and represents what I have always tried to do. The direct lecture has seemed to me a sometimes useful but always a disappointing pedagogical expedient. I have long wanted to attach string galvanometers to students who were enduring various forms of instruction as I did during examinations. In the absence of adequate data I conjecture that the student is more nearly asleep under the average lecture than at any other academic sacrament. The best lectures I have ever heard were those in which the listeners seemed to participate actively in the lecturer's problems, and in his analysis of the data which were available for their solution. I surmise that the problem method is the best available pedagogical technique all along the line. It seems to be closely analogous to the normal adaptation of the organism to situations as they develop in practical life.

The Mirror Tachistoscope. It was during the restless exploration of the eye-movements and their relation to reading at Halle, when tachistoscopic interests were very much alive, that I noted the reflection of illuminated objects from the surfaces of shop windows when the sun shone on the street. From this observation there developed the idea of a transparent mirror tachistoscope with reciprocal illumination of objects before and behind it. Construction was delayed until the Christmas vacation at Collegeville, and the original instrument whose construction littered our living-room still functions.

The Photographic Registration of Eye-Movements. During our discussion of the relation of eye-movements to the reading process Erdmann and I recognized the need for some sort of graphic registration. Notwithstanding active and persistent consideration of this new technical problem we found then no practicable solution. General suggestions of possible methods occurred to me, one of which finally led to photographing the corneal reflection. The path to successful records, however, was a long and devious one. It was not until I was settled at Wesleyan that the delayed reaction became overt. The development of the technique that is probably best known of all my work was full of characteristic examples of trial and error, approximation and correction.

I have reason enough for remembering the first plate. Knowing little of photography, I arranged to have it developed by a professional. It was exposed at the north window of my little ten-by-twelve office and laboratory. I had made a falling-plate camera with air-cushion control and expected to photograph the entire width of the exposed eyeball through a slit. I hoped to find bands corresponding to the iris and sclerotic and possibly even to the pupil and expected them to become oblique during eye-movement. The photographer reported "under exposed." We tried longer and longer development and more and more powerful artificial illumination. Faint bands came as encouragement, but, nothwithstanding careful focussing, the boundary between sclerotic and iris was regularly indistinct.

On some of the later plates I noticed a disturbing line that sometimes obscured the critical contours. It always moved in the same direction as the total eye-image but only about half as far. This disconcerting line was finally identified as a reflection of the light source at the surface of the cornea. It seemed to have possibilities. In their best form photographic records of eye-movements by the corneal reflection method isolate this line showing no other parts of the eye. The registration of human eye-movements is a field that I have made peculiarly my own. I believe that my techniques and experimental data are of permanent value. With the corneal reflection properly developed, Dodge and Cline measured for the first time graphically the angular velocity of the rapid, saccadic, or refixation movements and found them, for some still unknown reason, remarkably constant not only for the several angular deviations of the same person but also for different subjects.

For moderate angular deviations each saccadic eye-movement naturally divides into three phases. It starts from rest with a rapid acceleration, maintains an approximately maximum velocity for a central phase of varying extent, and passes into a phase of negative acceleration at the end, though it often shows an overshoot. In eye-movements of small angular deviation the central phase almost disappears leaving only the positive and negative acceleration phases. Yet the duration of normal saccadic eye-movement progresses quite evenly from 5° to 40° at the approximate rate of 10σ for each 5° of deviation. The data undoubtedly have important bearings on the dynamics of the action of antagonistic muscles, but that is probably a physiological rather than a psychological problem.

The approximate uniformity of saccadic eye-movements under normal circumstances suggested their use as indicators of abnormal neuromuscular conditions. The suggestion justified itself in a great variety of experimental situations. In fatigue they were found to become irregular in extent, direction, and speed, and gave important indications of the nature of relative fatigue, including the break. With moderate doses of alcohol they became slower, and proved to be one of the most regular of the neuromuscular changes effected by that drug. They changed with the phase of manic depressive insanity, speeding up in the manic and slowing down in the depressed phase. Later exploration in collaboration with Dr. Fox showed notable modification in certain abnormal conditions such as Friederick's locomotor ataxia and myasthenia gravis. In unilateral defective visual attention consequent to a brain tumor the usual long saccadic or refixation phases of pursuit nystagmus broke up into short "groping" refixation movements in the direction of defective attention.

There is still much concerning the saccadic or refixation eye-movements that we do not know in many other visual processes as well as in reading. The relative rôles of attention, intent, and short-lived motor habits are practically unexplored.

Varieties of Eye-Movement. The exploration of the eye-movements by photographic registration disclosed five clearly defined types of eye-movement: saccadic, pursuit, coordinate compensatory, reflex compensatory, and convergence. As we have already noted, the saccadic eye-movements are moments of changing fixation and interrupted vision. Their duration is shortest of all the varieties and is approximately constant for equal angles if the subject remains in the same condition. Pursuit movements are movements by which the image of a moving object remains fixed on the retina. Instead of presenting clear vision they are conditions of clearness, and within certain limits their angular velocity coincides approximately with the velocity of the moving field. The two are complementary and, since most visual objects move in relation to the head, both participate in most acts of vision. They are systematically combined in optic nystagmus—sometimes called "railroad nystagmus," since the alternation of slow and quick eye-movements is conspicuous when one watches the landscape from moving trains.

While our knowledge of the neural conditions of the saccadic movements is very limited, that of the pursuit movements is still more fragmentary. It presents a large and important field of research—important practically by reason of their relation to neural pathology. They are excellent indicators of malingering since it is beyond the power of the untrained subject either to simulate them or to inhibit them voluntarily when the object of fixation moves. Their modification in various cerebral and cerebellar lesions probably has important diagnostic indications. In the experiments of Diefendorf and Dodge they proved to be grossly disturbed in catatonic praecox and presented characteristic anomalies in other forms of sensory and neuromuscular disorders.

Just before the War I became particularly interested in recording the *reflex compensatory* eye-movements that occur when the semicircular canals are stimulated by rotation of the subject. The instrumental problem was peculiarly difficult and intriguing. Since a moving visual field is more or less in evidence if the eyes are open during rotation of the subject, reflex compensatory movements tend to be complicated by pursuit movements during direct observation, even when a strong convex lens is used to prevent clear vision. To study the reflex in pure form, the first requirement was to record the movements of closed eyes. It was one of the things that apparently couldn't be done. Ordinary photography by means of the corneal reflection was impossible. I thought of ultra-violet light and various other devices, but the danger and technical difficulties were for a long time insuperable. In exploring the possibilities of recording eye-movements I had noted the moving bulge of the lid as the eccentric cornea moved underneath it. The thing seemed to have possibilities. And even before successfully photographing the eye-movements I mounted a Marey tambour against that bulge. The records were not very promising, and I was deterred from trying to develop a technique both by the theoretical inadequacy of air transmission and by the reversal of the records after the apex of the cornea passed the central position. So the plan was abandoned as impracticable, but the bulge remained somewhere in the background of my thought and after long incubation got connected with the idea of a mirror resting on the lid. Proceeding, according to my habit, from rough approximations, I glued a little mirror to the lid of one of my advanced students and had the satisfaction of seeing it deflect a beam of light across the wall as the eyes moved. We found many limiting and disturbing factors, but the principle looked good and our little advanced laboratory class started in to develop the idea. The episode that participated in my explorations at Wesleyan. In this connection also I want to express my appreciation of the devoted and enthusiastic assistants among whom I am proud to include such names as W. F. Dearborn, Richmond, Newhall, Scofield, and R. C. Travis. Concave mirrors, more effective sources of light, slits, better cameras, and spectacle-frame supports were matters of direct engineering. The first frame still functions in the laboratory of the Yale Institute of Psychology. We took many exploratory records, and discovered the general character of the reflex compensatory eye-movement; the students then decided that they had settled all important problems of that type of eye-movement. This illustrates one of the conspicuous limitations of immaturity. The hypothetical projection of the known into the unknown as an experimental problem is a mysterious phenomenon at the heart of experimental procedure. Its investigation has long intrigued me. Verworn used to insist that knowledge is paralyzing and that only ignorance is stimulating. I feel confident that this is true, but only a partial truth. Stimulating ignorance is a particular kind. It becomes productive when combined with inventive restlessness and a certain amount of experimental tact. A study of the variability, the negative adaptation, and the relation of the vestibular to other data of rotation proved to Naval officers that the Bárány test is not a crucial test of capacity for aviation.

With the use of the mirror recorder in collaboration with Dr. J. C. Fox, of the Yale Medical School, an extensive program has been initiated to explore normal optic nystagmus and its pathological variations. The immediate objective is the early diagnosis of brain lesions. We hope that verified brain lesions will help to solve the riddle of neural innervation of the various types of eye-movement and their interplay. It is our hope that these and related studies may open the way for an experimental attack on some of the more fundamental problems of neural integration in the intact human.

The Conditions of Human Variability

One of the most stimulating scientific opportunities of the many which I have enjoyed was my appointment to the E. K. Adams Fellowship of Columbia University, 1916-18. It was a glorious opportunity for a long-desired, protracted experimental attack on the elementary conditions of human variability. The main problems had been more or less vaguely in my mind since the first clear records of long eye-movements when I discovered that notwithstanding their similarity no two successive eye-movement paths were exactly the same. I remember a long discussion with William James about the matter as we were coming from New York after a meeting of the American Psychological Association. He was neither surprised nor especially intrigued, but helpful as he always was to youngsters. He especially emphasized the general biological background of the observations. The problem of the conditions of normal variability began to take shape during the exploration of a normal knee-jerk, when I took records of the reaction of the simplest muscle groups which were available in the intact human and found them complicated by apparently endless modifying circumstances. It became definitely formulated during work at the Nutrition Laboratory of the Carnegie Institution when the neuromuscular effects of moderate doses of alcohol were under investigation. The theoretical background was presented in my Vice-Presidential Address of Section I of the American Association for the Advancement of Science. The basal experiments lasted over a year and a half and included a considerable variety of accurately recordable and relatively simple reactions from various levels of the neural system. When analyzed in systematic form these measurements showed the universal influence of refractory phase or something analogous to it at all levels of the nervous system and the differential influence of negative adaptation and new systematization at various levels.

Negative adaptation appeared to be the only true learning effect at the lower levels. Resystematization appeared in maximum degree if not exclusively at cortical levels. I conjecture that the interaction of these three processes and relative fatigue may go a long way in bringing order out of the chaos of human variability. Its repercussions on the theory of learning have been gratifying in quality if not in extent.

I do not know on what basis the Adams fellows are chosen, but I am desirious of adding a word of appreciation of the institution and its administration. It was an honor to be appointed and to have the results published in such notable company. I am very grateful to all concerned, especially to Dr. Woodworth and Dr. Pegram. And I am especially interested in the system by which adequate funds came into the hands of a seasoned experimenter in his own laboratory, without any red tape, to do a thoroughgoing and exacting piece of investigation. It seemed, and it still seems to me, the finest kind of scientific encouragement.

Just previous to my appointment as Adams Fellow, Columbia appointed me non-resident Lecturer in Psychology. I lectured on what was then a practically new subject, Problems and Methods in Dynamic Psychology, but instead of publishing the lectures I resumed the collection of data and am still not ready to generalize.

EXPERIMENTAL STUDY OF VISUAL FIXATION

This was my first extended monograph and was translated into German through the kindly enthusiasm of Professor Erdmann. As we grow older one of the great losses is that of the kindly interest and commendation of our old teachers. The best compensations are found in the affection and enthusiasm of our colleagues and pupils.

Aside from the technical discussion of the two photographic methods for recording eye-movements which were available at that time, the monograph contains the first experimental basis for my theory in regard to the law of approximation and correction and for the differential incidence of inhibition and stimulation in reading. My theory of the integration of retinal elements into a retinal system with implications in various fields has been pretty well lost. Perhaps it deserves to be, though I doubt it.

A SABBATICAL

One of the most fruitful years of my life was my first and only sabbatical in 1909-10 when I spent four months in Paris chiefly at the Marey Institute under the inspiring guidance of that great technician Dr. Lucien Bull, and five in Göttingen under the great physiologist, Max Verworn, who later became Silliman lecturer at Yale. They were joyous days.

My introduction to Dr. Bull still amuses me. I had made up my mind that I ought to become better acquainted with the celebrated work at the Marey Institute and its unique instrumentation. So I carefully polished up a few French phrases to introduce myself to the Assistant Director. I even practiced the name. After I had delivered my little speech, his face wreathed itself in smiles as he remarked with a reassuring English accent, "Oh, you're an American, aren't you?" My emotional reaction can best be imagined. I owe much to that instrumental genius. It was from him that I first learned the theory and technique of the string galvanometer and developed the idea of a parallel instrument, the microscope recorder, recently described by Miles.

For the second half of my sabbatical I wanted to study either under Sherrington or Verworn. It was Erdmann's admiration for the latter that finally decided me to go to Göttingen. The return to Germany was almost like a return home in spite of an amusing incident during our first meal at the hotel in Göttingen. We arrived on Christmas Eve and waited almost an hour that evening between soup and meat while the waiters at the hotel had their Christmas tree. Imagine it!

I was working primarily on mental fatigue and incidentally continuing my exploration of the knee-jerk. Verworn in particular was much amused at my facility in making instruments of precision from very humble materials. Mrs. Verworn reported his remark that he had found a man who could make better recording levers from bits of tin and a rubber band than his mechanic could turn out. The trait has served me well, though to be known as a rubber-band artist by my students has its disadvantages. I regard it as a maxim of scientific exploration that the use of simple instruments should precede the development of refined or complicated ones. Verworn regarded my experiments on the knee-jerk very highly and accepted my report in English for his *Zeitschrift für allgemeine Physiologie.* That was the only scientific publication for which I ever received an honorarium. Some of them have been expensive luxuries. It was during this sabbatical that I served as observer for G. E. Müller and came to know him as a friend. His methodological thoroughness and industry I have never seen surpassed.

Applied Psychology

My two excursions into applied psychology connected me with the alcohol program of the Nutrition Laboratory of the Carnegie Institution and with the national defense as member of the Psychology Committee of the National Research Council.

The experimental investigation of the effects of moderate doses of alcohol with the collaboration of Dr. R. G. Benedict occupied one of the busiest years of my life. My engagement as psychologist at the Nutrition Laboratory seemed to me a great honor and an intriguing recognition of the techniques which I had developed. It also seemed like a terrific scientific and social responsibility. The problem of prohibition was becoming acute, but knowledge of the psychophysiological effects of moderate doses of alcohol was conspicuously lacking. The whole prohibition movement rested on a debatable practical basis mixed with prejudice. Taken together with Miles's later confirmation, the scientific reliability of that study, as far as it went, left little to be desired, but its exhaustiveness was not of the same order. No one has yet gone far enough into the biological meaning of alcoholic depression of the neuromuscular processes or the balance of values which is represented by neuromuscular depression, on the one hand, and increased feeling of well-being, on the other. Decrement of reaction is sometimes highly advantageous, as, for example, in sleep. It is possible, moreover, that under certain circumstances the feeling of well-being may offset even a disadvantageous decrement. But it is clear that in these days of crowded highways and increasing breakdown of inadequately adjusted minds the purveyance or general use of anything that depresses adaptative adjustment invites social disaster.

At the end of the year of experimentation I was forced to decide whether to go on with the alcohol program at the Nutrition Laboratory or to return to Wesleyan and teaching. The decision seemed crucial. The reasons for and against staying were closely balanced. So I used once again my old standby—the Franklin calculus. It has never failed me and I never had a moment of regret for the consequent decision.

Miles had been engaged to take my place at Wesleyan for a year. His tactful management of my pet apparatus and his scientific resourcefulness impressed me so much that I recommended him to take my place at the Nutrition Laboratory. It was against the advice of the most famous of his teachers who felt that he was primarily a teacher rather than an experimentalist. As a matter of fact, he has proved himself to be pre-eminent in both fields and equally so in the field of friendship.

My second excursion into the applied field was during the Great War. It really amounted to a concentration of my entire scientific experience into a few months of agonizing exploitation. Probably no one else on the Psychological Committee except the chairman had the privilege of participating in so many phases of war service. I was a member of the original planning committee formed at the Spring Meeting of Experimentalists at Harvard, of the Psychology Committee of the National Research Council, of the Committee on Fatigue of the National Committee of Defense, and of the Committee on the Classification of Personnel in the Army. I was Chairman of the Committee on Vision and of the Committee on Psychological Instruction of the Psychology Committee, Psychological Consultant of the Chemical Warfare Service, and Consultant of the Training Section of the Bureau of Navigation of the Navy for the selection of listeners, and, at the end of the War, responsible for the psychological side of the Lookout School at New London as Lieutenant Commander, U.S.N.R.F. Naturally I was not equally effective in all these enterprises, but all were, I think, reasonably successful. I was glad of all the opportunities for national service but especially glad to be in the anti-submarine warfare which aroused me more than any other phase of the war.

One of the great moments of my life was when, after months of work as consultant, I found myself an officer of the U.S.N.R.F. for scientific service. I have a suspicion that my appointment transgressed many Naval traditions. The officers of the Navy with whom

I came in contact evoked my deepest respect and admiration. They were a group of much harassed but devoted and effective patriots. The distrust of them that I occasionally find in some quarters stirs me to vigorous resentment. Many of them were tackling jobs for which there were no precedents and were forced to deal with many unoriented enthusiasts and cranks. My first reception was characterized by tolerant courtesy. But as they gradually became convinced of our will and capacity to help without personal axes, their trust and loyalty and confidence in our ability to assist them knew no bounds. A delightful souvenir of those associations is a letter from Rear Admiral Palmer, which I venture to reproduce, since it outlines one of my major contributions.

NAVY DEPARTMENT
BUREAU OF NAVIGATION
WASHINGTON, D. C.

March 4, 1918

MY DEAR PROFESSOR DODGE:—

This Bureau is in receipt of an official report from the Commanding Officer of the Armed Draft Detail at the Navy Yard, New York, containing a description of an instrument devised by you for the primary purpose of selecting from among recruits those who are naturally fitted for training as gun-pointers.

The report indicates that in addition to fulfilling its primary purpose, the instrument has proven of great value as a device for training selected men, both as gun-pointers and gun-trainers. In this connection the report says in part:

> "This instrument has been in constant use now for over a month. During this time it has been found to be of great value, not only for classification of pointers, but further, for their actual training. Despite the almost constant use of this machine by different persons, no parts have become worn out or broken. It further has the most important advantage of being popular with the personnel who are being trained, and it has been found that the enlisted personnel make use of this instrument upon their own volition outside of drill periods. The motion of the target, derived by the design of this instrument, is by far the best the Commanding Officer has ever seen, and this opinion is supported by various other officers who have experimented with it. The diagram which is obtained from each pointer or trainer tells precisely how close to the target the man has kept during his period and further whether or not he fired when on. The records of these pointers or trainers are kept from day to day and one is soon able to tell whether or not the individual will ever pick up the necessary requisites for efficiency as such."

You may be further interested to know that the Bureau has taken steps to have the experimental instrument reproduced and furnished as a standard training device for recruits at all large training camps.

I beg to take advantage of this opportunity to express to you our sincere appreciation of the value of what you have done and are doing to assist in the very heavy burden imposed upon the Bureau in the present emergency: and may I add that we are also deeply sensible of your spirit of unselfish devotion to the cause which we all have so deeply at heart. You have given us most freely of your valuable time and have, I suspect, made other personal sacrifices of moment, and have declined to consider any method of compensation. I hope that this wholly unsolicited expression of the Bureau's gratitude will not be quite unwelcome.

I am taking the liberty of sending a copy of this letter to the President of Wesleyan University.

Again thanking you, I am

Sincerely yours,
(*Signed*) Leigh C. Palmer
Rear Admiral, U. S. Navy
Chief of Bureau.

Professor Raymond Dodge
7 Lawn Avenue
Middletown, Conn.

One minor detail of duty at the Lookout School in New London gave me considerable fun. We needed a simple disappearing target for training lookouts to spot periscopes. So I designed one on principles which were new to my fellow-officers. As they skeptically watched its construction some predicted it would go down and never come up, some, that it would never go down. To their amazement and my great satisfaction on the first trial it actually appeared and disappeared at will as they pulled one or the other of two tow lines. Their respect for psychology was much increased.

As far as it was permissible to publish them, a detailed report of my various wartime activities is included in the Report of the Chairman of the Psychological Committee of the National Research Council.

I am desirous of participating in the solution of one more problem in applied psychology. That is the problem of protracted human happiness. Whether or not it fits in with one's philosophy of life, the fact is incontestable that happiness is an important if not the most important aim of human endeavor. Notwithstanding this fact, it has received no commensurate scientific atention. The theory of the happy life remains at about the level where Greek philosophers left it. There has been an immense amount of ameliorative activity and human welfare work, but it is practically all a treatment of symptoms without fundamental analysis. We are trying to correct a number of the supposed major conditions of unhappiness. Personnel studies try to avoid putting round pegs in square holes. Studies of family life, like that of Hamilton's, try to develop adaptive behavior in the smallest social group. Medicine and hygiene try to cure and prevent ailments of body and mind. But these symptomatic and ameliorative activities touch only small sections of human unhappiness.

Scientific information as to the fundamental positive conditions of protracted happiness are conspicuous for their absence. We do not even know the laws by which pleasant situations become unpleasant and unpleasant situations become bearable or even pleasant. The positive conditions of happiness are left largely to accident, such as the satisfaction of instinctive wants with its tragic disillusionments and negative adaptions, the economic pressure to provide a market for manufactured products, the exigencies of the labor market, the desire to amass wealth, or the Bolshevistic abolition of private wealth. There are numerous wise protests that protracted happiness is achieved by none of those things; but positive, scientific data on the real conditions are as inconspicuous as scientific interest in the problem. If there were a real solicitude for intelligent adaptation, our science would be busy with very different tasks.

A Working Hypothesis for Inner Psychophysics

My "Working Hypothesis for Inner Psychophysics" was practically lost at the beginning by being classified in the *Psychological Index* under Weber's law. Somebody who didn't read the paper confused *inner* with *outer* psychophysics. I have never seen it referred to, though I believe that it was a legitimate extension of our experimental knowledge, and that it represents the most important insight I ever had. A popular version published by *Science News Service* had a better fate and didn't deserve it. The nucleus of the hypothesis is that intellective consciousness is neither an insignificant parallel of neural action nor a unique entity, but a special mode of integration that is independent of the stuff which is integrated, except as far as the stuff must be capable of that particular kind of integration. A corollary of that hypothesis is that evidence for this particular kind of integration anywhere in the universe constitutes evidence for intellective consciousness analogous to our own. This is no place for the argument on which the hypothesis rests, but I cannot conscientiously avoid mentioning it.

The Yale Institute of Psychology

At the end of the War my sympathetic friend, Dr. Shaw, asked me to write for the *World's Work* two papers on Mental Engineering during the War and after. I still regard those papers as essentially sound. Because it was the best existing approximation to my dreams of a College of Mental Engineering quite as much as because it seemed to offer freedom to develop my own program of investigation, I was easily persuaded to abandon undergraduate and other academic responsibilities to devote myself to the program of the Yale Institute of Psychology. The five years from 1923-28 have been very happy ones and, I believe, reasonably productive. Especially happy have been the personal relations within the Institute and with my colleagues in related fields in Yale University. Their sympathetic cordiality has been very gratifying. The scientific activities of this period are too recent for proper evaluation. The new Institute of Human Relations still more closely approximates my dream and I am glad to participate in its development. Naturally the tremendous responsibility of an undertaking of such magnitude oppresses me somewhat. As one of my friends wrote in a letter of felicitation, "If it doesn't make good it will hurt all of us." Personally I regard the Institute as a great scientific responsibility which can fully justify itself only with the help and cooperation of psychobiological scientists not only in this country but throughout the world.

My present scientific aims do not differ materially from the earlier ones. They may be expressed as a persistent effort to record with accuracy the behavior of normal and abnormal human organisms at various levels of neural integration, and to describe and understand that behavior as to its conditions, its variations, and its modification as the various levels interact to produce overt acts. I still remain primarily an experimentalist, but I am rather more interested than I once was in following the practical and theoretical implication of exact data as far as possible.

PIERRE JANET*

The editor of this collection had a very unique idea when he asked psychologists to write their own intellectual histories and criticisms, to transform themselves into philosophical historians, and treat themselves as though they had been dead for a long time. This hardly seems right since we are too active and too close to our own work to judge it with independence and to understand the influences which have unknowingly drawn us in certain directions. I have always protested against subjective psychology, and here I am asked for a most personal and subjective psychological analysis. It will necessarily be very poor, and the historians of the future, if by chance they should concern themselves with me, will find this autobiography very ridiculous. I beg to be excused in advance and I blame the initiators of this project and their powers of seduction.

I

I was born in 1859 and became interested in psychology at an early age. My studies seem to be the result of a sort of conflict, a compromise between incompatible and diverse tendencies. In my childhood I acquired a fondness for the natural sciences. At a very early age, I became interested in botany and started a collection of dry plants. Alas, since this is a confession, I must admit that I have retained that same unfortunate passion all my life. I still have my herbarium which I increase every year and which becomes increasingly cumbersome. This passion determined my taste for dissection, precise observation, and classification, which should have made a naturalist or physiologist of me.

But I had within me another tendency which was never satisfied and which one scarcely would recognize in its present metamorphosis. At the age of eighteen I was very religious, and I have always retained mystical tendencies which I have succeeded in controlling. It was a question of conciliating scientific tastes and religious sentiments, which was not an easy task. The conciliation could have been effected by means of a perfected philosophy satisfying both reason and faith. I have not found this miracle, but I have remained a philosopher.

My interest in philosophical studies was quickened by the example of my uncle, Paul Janet, my father's brother. Paul Janet, to whom I owe much, was an excellent man, industrious and intelligent, and today it seems to me that justice was not done him. He was not only a spiritual metaphysician, the last representative of the eclectic school of Cousin, but he was a great spirit who was interested also in politics and the sciences, and who, with great liberalism, sought to reunite these studies. He understood the importance of medical and anatomical studies to the moral intelligence of man. It was he who, at the beginning of my philosophical studies, presented me to Dastre, Professor of Physiology at the Sorbonne, and started me in his laboratory. It was he who had me, after normal school, enroll at the Medical School in Paris and continually urged me to combine medical and philosophical studies.

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*Submitted in French and translated for the Clark University Press by Dorothy Olson.

Already the philosophers of Cousin's school, Maine de Biran and Joufroy, regarded psychology as a departure from philosophy, and my philosophical ideas, at once scientific and religious, led very naturally to a study of psychology which was to terminate in the distant future in the desired metaphysics. Do not the thousands of observations on the ideas and sentiments of the afflicted and of those presumably in good moral health, which I have gathered during my whole life and classified with so much care, constitute a collection, a herbarium, which may be placed alongside of the other? Under such diverse influences, the philosopher has become a psychologist.

When I became Professor of Philosophy at the Lycée at Havre at the age of 22, I wished to continue my studies of medicine and scientific psychology in spite of difficulties. I was received with a welcome, which I shall never forget, by the doctors of the hospital who put themselves at my disposal not only in communicating medical experiences to me but also in procuring subjects who were interesting from the psychological point of view.

An unusual proposition made by a well-known doctor in Havre, Dr. Gibert, has from the beginning oriented my studies in a rather unforeseen manner. At that time, it was my intention to prepare a medical thesis on hallucination and to study in connection with this the mechanism of perception. I asked Dr. Gibert if he knew of anyone suffering from hallucinations that I might study. He told me that he knew of none at that time which was interesting, but that he could show me other psychological cases which in his opinion were far more remarkable. He had always had a certain partiality for the study of animal magnetism, which had flourished in Normandy, above all in Caen, and which persisted despite official discredit even of the connoisseur. He had kept in touch with a woman known by the name of Léonie, who had been hypnotized in her youth by Dr. Perrier of Caen, who had been introduced by Dupotet, and who had been observed to perform some curious things with clairvoyance, mental suggestion, and hypnotism from a distance, etc. What a godsend for a young psychologist, 22 years of age, curious as to all psychological phenomena and drawn by the mysterious side of these occult faculties! At my request Gibert had the celebrated Léonie brought to Havre and my studies on her at various periods over a stretch of years oriented my early works toward the marvels of hypnotic somnambulism.

II

The experiments that Gibert showed me and that I myself reproduced on Léonie, in particular the provocation of hypnotism from a distance, did not seem entirely conclusive but were, nevertheless, quite strange and worthy of attention and discussion. I had the opportunity of informing of my work a society of psychologists just recently founded in Paris under the presidency of Charcot and Charles Richet. This little discourse, though very prudent and skeptical as to mental suggestion and hypnotism from a distance, nevertheless attracted the attention of the Society for Psychical Research in London who proposed to send one of their members to Havre to verify my work. The experiments which I conducted at the request of this commission and with the precautions demanded have given some very interesting results: 16 times out of 20 somnambulism has exactly coincided with a mental suggestion made at a distance of one kilometer. These experiments, which the representatives of supernormal (*supra-normale*) psychology have published and popularized in my opinion too soon, have since that time been cited and used in all works on the unknown faculties of the human mind. In viewing these citations and this abuse of my former observations, I have always had a feeling of astonishment and regret. Strange that these authors who reproduce with such confidence these experiments of 1882 have never had the idea of writing to the experimenter who was still living and asking what he thought of them! I should have answered that already at that time, and even more so now, I doubted the interpretation of the facts and was disposed to criticize them myself, regarding them as a simple departure from more profound studies.

My first entrance into the study of the disorders of the nervous system by examination of mysterious phenomena and doubtful reality does not seem entirely regrettable. In the first place, these strange investigations have put me in contact with some important people who had the same curiosity at the back of their minds, Charcot, Charles Richet, Frederick Myers, Sidgwick. They have informed me of their own enthusiasms and doubts, and have shown me their own research work and methods. This difficult and dangerous research work has taught me from the beginning the necessity of a certain disposition of mind indispensable for the study of pathological psychology. One must approach this research with a certain calmness devoid of systematic and predetermined admiration or denial. Charcot said to me later in speaking of the study of hysteria: "*Nil admirari* is an indispensable attitude." I had already learned that in examining Léonie. I was very much displeased after each séance to hear the exaggerated and inexact accounts of the assistants who appeared awkward and talkative during the experiment, and afterwards constructed entirely false recollections of what had happened. At that time, I resolved to examine subjects and patients as far as possible without the encumbrance of witnesses. Furthermore, I acquired a habit which I have always retained, the habit of writing constantly during the meeting minute notes on everything that happened, of noting the words spoken by the witnesses, by the patient, by myself, and keeping no further account of any recollection unless it coincided exactly with some written note. My psychology has become the "psychology of the fountain pen," and my descriptions of the patients have unfortunately become unusually long and weighted by the reproduction of the exact words spoken and recorded by me. However, all this gave to the observation the character of reality which I sought particularly. Whatever they may be, these first studies in the wonders of animal magnetism turned me to the study of somnambulism and hypnotic practice, which were then very popular and at least appeared to be a means of approach to the psychological study of mental pathology.

It must be remembered that at this time, in 1882, I had made only very few studies in anatomy and physiology and I had not the slightest notion of the teaching of Charcot at Salpêtrière, nor of those of Bernheim at Nancy of whose very existence I was ignorant. With some difficulty, I became acquainted with these teachings and, at the same time, compiled the works of the French hypnotists into a collection which is still of interest today. Entirely independent of these various schools, I set about criticizing these works. I proved in particular the very curious and historical relationship between the teachings of Charcot or Bernheim and those of the hypnotists whom they pretended to ignore and scorn but who nevertheless influenced them. (*Médications psychologiques,* Vol. I, p. 170.) My conviction was that these authors approached the study of certain strange forms of behavior with too much medical preoccupation and without sufficient knowledge of the psychological problems underlying these conditions. This led me to an extended study of neuroses, particularly hysterical neuroses, which I continued at Havre until 1889 and at Salpêtrière in the Psychological Laboratory which Charcot had placed under my supervision in 1889. This work has been summarized in several articles published since 1886, in my philosophy thesis, 1889, and in my medical thesis, *L'état mental des hystériques,* 1892.

These studies have been somewhat forgotten today because of the discredit thrown on observations relative to hysteria since the death of Charcot in 1895. Hysteria patients seemed to disappear because they were now designated by other names. It was said that their tendency toward dissimulation and suggestibility made an examination dangerous and interpretations doubtful. I believe these criticisms to be grossly exaggerated and based on prejudice and misapprehension, and I still am under the illusion that my early works were not in vain and that they have left some definite ideas.

From the psychological viewpoint, they have to a small extent begun to throw light on the difference between actions of the higher centers and those of lower order, between synthetic and automatic acts. The latter were only the regular repetition of a group of psychological phenomena, of a system of ideas, images, emotions, movements, which had been set up by the higher acts of synthesis at the moment when a complex situation presented itself for the first time. This difference, especially in certain cases, gave rise to the distinction of *unconscious* acts as opposed to completely *conscious* acts. These studies have begun the interpretation of *suggestion* which plays so important a rôle in social behavior and have approached those of *will and belief*.

From the medical viewpoint, I still believe that one will eventually be compelled to return to interpretations of neuropathic disorders similar to those which I have proposed in regard to hysteria. I was one of the first to point out the enormous rôle of suggested beliefs and autosuggestion in hysteria: in my thesis on the mental state in hysteria, 1892, I designated most of the accidents of the neurosis by the name of fixed ideas of the hysteriac. After the death of Charcot, an interpretation of hysteria which reattached all the symptoms to suggestion was presented with the pretension of being entirely different from mine. Of course, in reality I had never absolutely systematized my interpretations by means of fixed ideas. In establishing the importance of suggested belief in hysteria, I was forced to notice that this explanation was not to be exaggerated, that even in certain cases of hysterical hemiplegia, there was a disposition to go beyond the fixed ideas of the subject, and that in general these fixed ideas did not always exist with precision. Above all, I could not consider this tendency toward suggestibility as an absolutely primitive phenomena, I could not admit that an ailment might be explained by limiting one's self to saying that the subject had suggested to himself that he was sick. In my opinion, a preliminary ailing tendency, a weakening of the functions of resistance and synthesis, are necessary to give rise to suggestibility.

In seeking the conditions of this weakening which in my opinion are numerous, I was led to recognize in certain cases the rôle of one or several events in the subject's past life. These events, which had established a violent emotion and a destruction of the psychological system, had left traces. The remembrance of these events, the mental work involved in their recall and settlement, persisted in the form of lower and more or less conscious psychological processes, absorbed a great deal of strength, and played a part in the persistent weakening. Here still, if I am not mistaken, this notion has been fruitful and has given rise to a whole theory of neurosis and psychosis by the subconscious persistence of an emotional traumatism, and a whole method of research has been worked out to the utmost of this kind of traumatism. So far, I had never introduced a clinical observation as a metaphysical system, and I had never claimed that all neuropathic weaknesses were exclusively the consequence of a traumatic reminiscence. Besides, my studies at the Salpêtrière showed me more and more the part played by exhaustion of all kinds, organic ailments, and hereditary predispositions; I did not want to exaggerate the import of a just observation in some particular cases.

III

This fear of generalizing a particular observation, this desire to point out the different forms of psychological weakness have led me to a study of other aspects of depressive neurosis. In a large hospital where there were so many different kinds of patients, it was easy to choose, and for several years I devoted my studies to tics, insanity, phobias, obsessions, and impulses of all kinds. This new series of observations is summarized in numerous articles, in my books on *Les névroses et les idées fixes,* and has terminated in my work on *Les obsessions et la psychasténie,* 1903.

These various disorders which torment the patient for years and which border on grave mental maladies, dementia praecox in particular, had been described separately without relation to one another. I have sought to give some unity to this confused group of symptoms in discovering what is essential and common to the different forms of disorder. That has permitted me to place under the heading of *psychasthenia,* if not a so-called malady, at least a great syndrome, a form of neurosis, distinct from epilepsy and hysteria.

In my description of the symptoms of the psychasthenic neurosis, I stressed particularly the pathological feelings (*sentiments pathologiques*) which I designated at that time as feelings of inadequacy (*sentiments d'incomplétude*) and which have become in my last book a part of the *feelings of emptiness* (*sentiments du vide*).

From the psychological viewpoint, this work on obsessions and psychasthenia marks a very interesting stage in the evolution of my ideas on the different degrees of psychological activity. Instead of limiting myself to two easily distinguished forms, automatic activity and synthetic activity, I have been led to establish certain degrees of higher activity. One quality of these activities strikes the observer when he examines the maladies of doubt, the various aboulias, and the feelings of inadequacy (*sentiments d'incomplétude*). In the normal man, these activities are characterized by a strict conformity between actions and exterior reality, physical or social. No doubt, generally speaking, all activity conforms somewhat to reality: the simplest reflex is adapted to some fact in the real world, but this relationship which is not perceivable in the lower activities becomes at a certain level an object of consciousness, and this feeling of reality plays an important part in the operations of the will and belief. It is easy to recognize that most of our patients have difficulty with this function of reality.

In studying these differences in functioning, one is naturally led to suppose that all the operations of the mind do not have the same degree of facility, and that in the course of a weakening of cerebral functions, they disappear successively and progressively by reason of their unequal degrees of facility. The degree of *psychological tension* or the *elevation of the mental level* is manifested by the degree in the hierarchy of phenomena occupied by the highest functioning which the subject attains. The functions of reality, together with actions, perceptions of reality, certainty, all demanding high degrees of tension, become phenomena of high tension; dreams, disturbances of motility, emotions demanding lower tensions may be considered as phenomena of low tension corresponding to a lower mental level.

IV

I wish to mention here a circumstance, which, if I am not mistaken, has played a certain part in the evolution of my teachings. Placed by circumstances between philosophers and medical men, for a long time I had the feeling that it was very difficult to speak to both at the same time and that their different languages prevented them from understanding each other. I have never been so struck by the language difficulty in psychology as in a trivial incident which I beg your permission to repeat.

About 1896 or 1897, some of the students at the hospital, the internes, and the directors of the clinic among whom were my friends, Laignel-Lavastine, and the lamented Sicard, came and asked me to conduct a special course in clinical psychology for them. At the end of the course, a strange thing happened: the students were satisfied or at least were kind enough to say they were, but the professor was very much dissatisfied with himself. Never before had I had such a feeling of the insufficiency of this teaching, the conventional character and practical nullity of our psychology. At that time, having begun to substitute for Ribot at the College of France, I had the opportunity to work up the course in a new way and with new terminology. During my thirty years at the College of France, I believe I treated all questions of psychology and psychiatry from a more clinical point of view and with a more appropriate terminology.

A practical psychology which aspires to a part in jurisprudence, pedagogy, and medicine should above all be objective and based upon externally observable facts. Psychology evolved from Cartesianism regarded thought as the most primitive phenomenon and action as a consequence or secondary expression. Its language is based upon descriptions of internal phenomena and is not in accord with the language based upon objective descriptions. We are obliged to formulate a psychology in which externally observable action is the fundamental phenomena, and in which inner thought is only a reproduction, a combination of these outward actions in a reduced and specialized form.

Behaviorism, so necessary in the study of the conduct of animals, may easily be applied to elementary acts. The question arises: Can the same kind of description be applied to the behavior of men? It is possible under two conditions: in the first place, this psychology of action must make a place for consciousness, which may be forcibly suppressed in the case of lower animals. One must regard the phenomenon of consciousness as specialized conduct, a complication of the act which is superimposed on the elementary conduct. A second condition is that in this description of conduct one must necessarily be preoccupied with the higher forms of conduct, beliefs, reflection, and experiences. These facts have ordinarily been expressed in terms of thought, and in order to preserve the same language throughout the science of psychology, it is necessary to express them in terms of action. This psychology may be designated by the name *psychology of conduct* in order to indicate that it is concerned with a broader and higher form than behaviorism. This is what I have been trying to do in my teaching for the last thirty years.

These reflections determined by the necessity of clinical teaching have inspired my later works. In the three volumes of *Médications psychologiques* in 1919, I presented in this more precise language a summary of my long medical studies on neuroses, psychoses, and their treatment. Not only did I try to review the doctrines which I had gathered since my youth on the history of various methods of psychotherapy, but I also tried to explain briefly the facts and ideas contained in those words repeated so often at random, *suggestion, hypnotism, moral catharsis* (*désinfection morale, liquidation morale*) *rest, aesthesiogeny, isolation, excitation, moral direction.* I have stressed a subject which has always interested me, that of the difficulty of social action. It has not been taken sufficiently into account how much one person by words or his presence alone can modify in one sense or another the psychological tension of another. The problem of religious conduct is closely related to this study of influences, directions, and social excitation.

V

This psychology of conduct, however, presents difficulties and raises new problems. Many of the higher psychological phenomena have an internal spiritual aspect and appear entirely different from so-called actions. The desire to classify all psychological facts under action and conduct has forced me to introduce a new analysis of *consciousness, belief, memory, thought,* and above all *emotions.* These studies were explained in my courses on *inner thought* and on the *evolution of memory and the notion of time,* both of which have been published and in my two volumes, *De l'angoisse à l'extase,* 1928, which deal with belief and emotions. Thought is inner language; belief becomes a special combination of language and action; memory is above all a system of recounting; emotions are regulations of action, reactions of the individual to his own actions.

The psychology of conduct adapts itself very readily to our former conception of psychological tensions which places one tendency above another according to its degree of complexity, perfection, and order of acquisition. In my lectures at the university, and in my first volume of *De l'angoisse à l'extase* I was able to give a pictorial hierarchy of the more definite tendencies. I was glad to notice that from this point of view the different forms of contemporary psychology, child psychology, and psychology of primitive peoples, all agree along many lines with pathological psychology and approach an analogous pictorial hierarchy. I have particularly stressed two forms of belief, primitive belief (*assséritive*), and reflected belief: this distinction seems important for the interpretation of suggestions and delirium.

At the same time, the psychology of conduct obliges us to stress another aspect of actions which often appear in a different form and have another efficacy although they seem to remain at the same level. The efficacy of actions appears to depend not only on their psychological tension but also on the material force of movements capable of displacing objects, on the rapidity of these movements which determine the displacement in a given time, on the relative duration of these movements. Those are the measurable quantities which express the energy of a living being. Instead of trying to introduce quantity in psychology by the hypothetical constructions of psychophysics, one must introduce quantity by the appreciation of the energy of the subject and its variations.

Modifications of psychological energy, whether they be general or more or less systematized in a specific tendency, determine great changes in character and play an important part in most psychological disorders. At the point of departure from the neuroses, one can ascertain hereditary exhaustions, exhaustions of infectious or toxic origin, and in many cases particular actions which have caused the exhaustion. I have drawn up a list of these costly actions and of the characteristics of the action which modify the expenditure of energy. Many disorders have a direct bearing on the derivation of energy which is produced when a more or less charged action of high tension cannot be executed. The psychological problem of the cost of action, of exhaustion by expenditure, of the use of residual energy will later become a paramount problem in psychology and psychiatry although today it is scarcely suspected.

I must mention a whole new study which has scarcely begun and which has as its object not only this psychological energy but also the relationship between this energy and tension. I have approached part of this study in examining the phenomenon of *discharge* which plays an essential part in convulsive attacks and in the *crises of psycholepsy.* Probably in normal life, among well-balanced individuals, a certain proportion must be maintained between disposable energy and tension, and it is not good to combine great energy with feeble tension or inversely to seek to maintain high tension with insufficient energy: the result is always agitation, insufficiency, and disorder. The most useful psychology of the future will be a practical psychology of conduct which will at the same time be dynamic and will study the physiological production of energy and its distribution.

Without doubt, these systematic constructions are very hypothetical and temporary. The most interesting part of my work will always be the numerous observations I have gathered on both the normal and ailing man. I should never have been able to gather them or classify them if I had not been directed by philosophical ideas which were always indispensable. As William James said, one sees what one is prepared to see, so too, one cannot study the psychology of man without guiding ideas, without philosophical or even religious interests.

JOSEPH JASTROW

An account of one's professional development may proceed by the double route of formal discussion and an informal narrative. Graduated from the University of Pennsylvania in 1882, I found myself with more interests that I could eliminate from a choice of career than with a definite decision. The college course of those days was substantially of the older rigid type. The teaching was likewise of a rather doctrinaire fashion. There were few professors who offered much in the way of stimulation. The undergraduate course had slight bearing on one's professional ambitions. I wrote the senior prize essay upon the subject of philosophy (Leibnitz), and as a graduate student in this subject enrolled in The Johns Hopkins University in the fall of 1882.

1882-1885. Professor George S. Morris of the University of Michigan spent half the year at Johns Hopkins giving courses in philosophy, and Professor G. Stanley Hall took his place in psychology for the other half year. It was Charles S. Peirce, one of the most exceptional minds that America has produced, who stimulated me most directly. He was connected with the U. S. Coast and Geodetic Survey. The son and brother of distinguished mathematicians at Harvard, he was himself a mathematician of first rank. He was then concentrating upon logic—the algebra of logic—a science founded by George Boole in England and Schroeder in Germany. Peirce's lectures impressed the student as an amazing exhibition of mental ability. Logic in Peirce's hands covered not merely a development by means of mathematical formula of the machinery for solving problems, but became an investigation of the nature of the thought processes, including their psychological foundation.

The first psychological investigation made at Johns Hopkins University was likewise undertaken at Peirce's suggestion. He was interested in the problem of the smallest perceptible difference of sensation. It will be recalled that the establishment of a psychological laboratory by Wundt in Leipzig (1879) was preceded by many years by the work on this topic of Gustav Fechner (*Elemente der Psychophysik,* 1860), which in turn was based on the still earlier studies of E. H. Weber, who, however, regarded the problem largely as one of physiology in connection with his exploration of the tactile senses.

The problem which Peirce proposed was that of determining whether if after a difference between two stimuli was reduced below the so-called threshold, and one continued to guess which of the two weights or pressures was the heavier, which of the two surfaces the brighter, one would still guess correctly in proportion to the actual (but subliminal) difference present. If the threshold were a physiological limit below which there would be no registry whatever, there should be no difference in the proportion of right and wrong guesses in such slightly different stimuli. But if the proportion continued to diminish below the threshold, the fact would argue for a subconscious registration. Such proved to be the case; later studies by other methods have confirmed this conclusion. The study is interesting as a pioneer contribution in American experimental psychology. The memoir appeared under the joint authorship of C. S. Peirce and myself with the title "Small Differences of Sensation," in the *Proceedings of the National Academy of Sciences* (Oct. 17, 1884); and we participated equally as subject and observer. The pressure-balance devised for the investigation is the forerunner of all the improved pressure-balances since employed. With the coming of G. Stanley Hall (1883), systematic lecture courses in several branches of psychology were established and a psychological laboratory installed. I at once enrolled as a candidate for a professional degree in psychology, though I continued my studies in logic.

1885-1888. The variety of the interests which I cultivated in student days at The Johns Hopkins University is more readily indicated by the published papers of that period than by my present recollections.

"Some Peculiarities in the Age Statistics of the United States." (*Science,* June 5, 1885.) A study showing the tendency to report ages in round numbers, the growth of this tendency up to age 60 (50 for the colored population, in whom the tendency reaches gigantic proportions), its greater prevalence among women, its minimum in the native white population.

"Composite Portraiture." (*Science,* August 28, 1885.) A general account of the recent invention by Galton and the suggestion of combining any two photographs by means of a stereoscope.

"Studies of Rhythm." (Jointly with G. Stanley Hall.) (*Mind,* January, 1886.) A study of the time-sense by intervals between auditory clicks, and of the rhythmic function in its perception.

"Elementary Science Teaching." (*Science,* February 5, 1886.) An account of early atempts at object lessons and popularization.

"University and College." (*The Pennsylvanian,* March 9, 1886.) An advocacy of graduate study when that was an innovation.

"On the Symbolic System of Lambert." (*Journal of Speculative Philosophy,* April, 1886.) An early precursor of symbolic logic; an historical study instigated by Peirce's course in logic.

"On the Existence of a Magnetic Sense." (Jointly with George H. F. Nuttall.) (*Proceedings of the American Society for Psychical Research,* July, 1886.) Preceded by a historical introduction concerning the belief in a magnetic sense, this study recounts a careful investigation of the possibility of detecting whether the current were on or off when the human head was placed between the poles of a large electromagnet. The results were completely negative. A pioneer experimental study to test beliefs in unusual human powers which have so largely occupied the movement known as "psychical research." (Dr. Nuttall, F.R.S., is now Professor of Bacteriology at Cambridge University, England.)

"An Easy Method of Measuring the Time of Mental Processes." (*Science,* September 10, 1886.) The first use of chain-reactions in systematic form.

"The Perception of Space by Disparate Senses." (*Mind,* October 1886.) My Ph.D. thesis. An experimental study of the correlation of the visual sense, the joint-sense, and the kinaesthetic sense in finger movements, proving the dominance of the visual perception.

"The Longevity of Great Men." (*Science,* October 1; reprinted in *Nature,* November 4, 1886.) Showing the relative life span of eminent men of thought, of feeling, and of action. This was an outcome of a study of great men conducted by Mr. Peirce, who associated with him a group of graduate students with various interests.

"The Psychophysic Law and Star-Magnitudes." (*American Journal of Psychology,* November, 1887, the first isue of the journal founded by G. Stanley Hall.) The study showed that in the assignment of magnitudes to stars by estimated brightness, there is a definite tendency to follow a series of relative brightnesses and thus to follow the psychophysic law. The experimental confirmation was made under the direction of Peirce.

"The Dreams of the Blind." (*New Princeton Review,* January, 1888; reprinted in *Fact and Fable in Psychology,* 1900.) A study of the dreams of blind persons showing that the critical age after which visual imagery is maintained is from the fifth to the eighth year.

"A Critique of Psychophysic Methods. (*American Journal of Psychology,* February, 1888.) An evaluation of the utility, accuracy, and applicability of the standard methods for determining the sensibility of the senses.

"Genius and Precocity." (*The Christian Union,* March, 1888.) This study was likewise an outcome of the study of great men under the direction of Peirce, showing an unquestioned relation between the possession of unusual mental powers and the early development of precocity.

"A New Aesthesiometer." (*American Journal of Psychology,* May, 1888.) The parent model of the standard aesthesiometers still used.

"Genius and Precocity." (*Journal of Education,* July, 1888.) A comparison of my results (see above) with those obtained by Sully.

"Eye-Mindedness and Ear-Mindedness." (*Popular Science Monthly,* September, 1888; reprinted in *Fact and Fable in Psychology,* 1900.) A popular study of the dominance of eye and ear in mental assimilation; the first use of these terms for which the French used *visionaire* and *auditaire.* Galton had studied the subject in England.

"The Psychology of Deception." (*Popular Science Monthly,* December, 1888; reprinted in *Fact and Fable in Psychology,* 1900.) A popular study of the techniques of deception, including those of the conjuring stage and the claims of spiritualistic mediums. The development of this interest and further contributions led to the publication, in 1900, of *Fact and Fable in Psychology,* a subject to which I have continued to contribute to the present day.

What stands out notably in the impression of years concerning the leadership of Stanley Hall is the encyclopaedic sweep in the subjects of his interest, and the variety of the disciplines that he mastered. From the outset his *Fach* was education (or pedagogy, as he preferred to call it), as well as psychology; the title of his chair included both subjects. With the shifting emphasis of the years and with the refinement of methods in psychology, there has been a tendency to disparage the value of his contributions. He has been regarded as uncritical or even credulous, sampling indiscriminately all varieties of contribution and attempting a synthesis lacking perspective. This criticism refers to his major writings as well as to his use of the questionnaire method at Clark University, and to his adherence to the recapitulation theory. Hall deliberately chose this procedure upon the principle that suggestive hints for investigation arise from a composite view of a subject from all angles. Despite the weakness of the method, its use in Hall's hands resulted in a broad approach and a stimulating effect upon his students. His influence as well as his contributions place him in the small group of the founders of American psychology. They equally reflect a mind of extraordinary powers. The child-study movement, the proper appreciation of the genetic principle, the inclusion of the abnormal as an integral illumination of normal phenomena, remain as evidence of his pioneering insight. His share in establishing psychology among the recognized sciences is a notable one.

The group of students whom he directed came to him with varied purposes. Two of these with whom I was intimately associated at Johns Hopkins became Stanley Hall's life-long associates at Clark University, when he accepted the presidency of that institution, founded in 1887. These were Edmund C. Sanford, who became Professor of Psychology, and William H. Burnham, who became Professor of Pedagogy. The first fellow in psychology was James McK. Cattell who left Johns Hopkins to continue his studies at Leipzig. Among the philosophical group with strong interests in psychology were John Dewey, G. F. W. Patrick, and George W. Hyslop.

There were others whose major interest was in biology. The psychological seminar was thus a gathering of men of varied interests centering about psychology. It was the first group professionally headed for psychology as a career. The first Ph.D. given specifically in psychology was conferred upon me in 1886, as Cattell had left, and the other degrees that preceded my own were conferred in philosophy. This professional spirit was emphasized by the foundation of the *American Journal of Psychology*, providing a place for the publication of the results of research. The close relations of psychology with the other sciences were aided by the housing of the laboratory in the biology building. That the biological approach to psychology is an essential foundation is today a commonplace; but at that period it was an innovation. Students in psychology were expected to be grounded in physiology, and took part in Professor Martin's course in physiology, specializing also in neurology. Henry H. Donaldson, then Professor Martin's assistant, was also a worker in the psychological laboratory and served as an intermediary between the two departments.

The problem that intimately concerned the graduate student was that of securing a position. There were as yet no teaching positions in psychology alone. Psychology was associated with philosophy, and the smaller colleges were likely to continue that tradition. That other institutions might be stimulated by the example of Johns Hopkins University was a hope with slight promise of immediate fulfillment. The earlier and enthusiastic advocacy of psychology as a special province by William James was an important aspect. James's interest lay in the larger interpretations of psychological principles. He had a certain impatience with minutely tedious experimenting, especially that based on the Teutonic model. Yet he was convinced that many of the problems of psychology could be approached favorably through experimental study. The first studies of this kind in this country, though sporadic, were made under his direction at Harvard University. His classic *Principles* gives generous credit to the younger workers. The same conviction was expressed a few years later at Yale University by George T. Ladd. Though a philosopher and brought up in a wholly different tradition, he realized the importance of the new experimental approach, and was the first to provide a text for this study. Ladd's *Elements of Physiological Psychology* (1887) should be brought into the record of this period.

Yet all this encouragement did not provide any tangible opportunity for a career in psychology. Accordingly, I continued in 1886-87 as a fellow-by-courtesy, and continued my investigations and studies. I prepared articles for *Science* and for the popular magazines. The situation remained much the same in 1887-88. I then drew up a prospectus of a short course of lectures, including several of the more attractive problems covering the newer phases of psychology. Accompanied by recommendations, I sent these to a selected group of colleges and universities that gave promise of hospitality to the new discipline. My proposal was accepted in the spring of 1888 by the newly elected President of the University of Wisconsin, the geologist, Thomas C. Chamberlain. His progressive attitude was responsible for my appointment, as similarly a few years later he invited Professor Richard T. Ely, Professor of Economics at Johns Hopkins, to transfer his activities to Wisconsin. I gave this course of lectures at Madison in the late spring of 1888, and was invited to found a chair in psychology in the fall of that year. The tradition at Wisconsin was the same as elsewhere, but had the advantage that the former president of the University, President Bascom, who had given the major courses in philosophy and psychology, was a man of distinction with a strong psychological interest. The only parallel venture at the time was that of the University of Pennsylvania, which had invited Cattell to a similar chair in the previous year; and next Bryan was invited to found a Department of Psychology at Indiana. It was in

1888 a bold step for a state university to add psychology as a special study; the next ten years proved its justification. Such chairs included a laboratory, an acceptance of the idea that psychological problems required instruments. For this the example of Johns Hopkins University was responsible.

1888-1893. In inaugurating the work at Wisconsin, I felt the handicap of the lack of acquaintance with the European institutions—a background which I deemed important for a professional career. A leave of absence was arranged for in the spring of 1889 for the purpose of visiting the chief European centers of psychological activity. I also attended the first International Congress of Psychology at Paris. The record of this visit appeared in three articles published in *The Open Court:* "Psychology in Germany," "Psychology in France and Italy," and "Psychology in Great Britain and the United States." These were later published under the title, "Aspects of Modern Psychology," in a small volume called *Epitomes of Three Sciences,* 1890.

The organization of a Department of Psychology in those days presents a sharp contrast to the present conditions. There was no text available. The only compilation was a book rich in information called *Mental Physiology,* by William B. Carpenter, a pioneer attempt to interest the public in a variety of problems which had a psychological aspect. I used this as a text to indicate a type of material at least in harmony with the new psychology. I supplemented it with a book on the physiology of the senses; for the study of sensation was the natural point of departure of the experimental work.

Apparatus was largely home-made; and it was, I believe, the commission to build a piece of apparatus for me that started the work of Stoelting and Company on their career as makers of psychological apparatus. The problems chosen were those developed at Johns Hopkins, which in turn followed somewhat the model of Leipzig. The early volumes of the *American Journal of Psychology* contain a number of studies from the new laboratory at Wisconsin. From the outset there was a laboratory course paralleling the lectures in Experimental Psychology, and thus providing the students with an opportunity to demonstrate for themselves procedures and methods underlying conclusions. As soon as Sanford's *Course in Experimental Psychology,* Part 1, was available, I used it as a laboratory text.

An important factor in the dissemination of popular interest in psychology of this period came from Francis Galton, who approached the subject as an anthropologist, yet whose studies in *Hereditary*

Genius showed a versatile interest. His *Inquiries into Human Faculty* is definitely a psychological treatise. It led him to parallel the various instruments and methods for measuring bodily traits by similar instruments for mental traits. He called this "anthropometry" or "man measurements" and introduced, in this group, determination of the speed of reactions, color sensibility, pressure sensibility, and many another "test." To Galton belongs the credit of the testing laboratory as a means of exploring the varieties of human endowments.

Under this general influence, an exhibit of the new psychology was arranged at the Chicago World's Fair of 1893, which I was invited to undertake. I arranged cases of apparatus with descriptive labels to render visible to the public the nature of the problems which psychologists were considering. There was also a testing laboratory at which individuals for a small fee could have their sense capacities and mental powers tested, including a number of experiments or observations in which the subject made his own record.

The exhibit aroused popular interest; it stands as the first attempt to introduce tests to the American public. A considerable number of observations were collected but they were not published. The only record of this venture is in the guide books of the World's Columbian Exposition.

The work of instruction at a state university with rapidly increasing numbers of students and a small faculty was arduous. I gave courses in Logic as well as Aesthetics and even a course in Greek Philosophy. I had no assistants, and conducted the laboratory as well as the lecture and class work single-handed. After five years, the laboratory was established in better quarters, and the courses in psychology had grown to such an extent that I could specialize in that field, though I continued by choice the course in Aesthetics. The elementary work was from then on divided, part of it falling to the instructors in philosophy.

1893-1926. At a meeting of the American Psychological Association held in Madison in 1922, the following resolutions were adopted:

> WHEREAS, Professor Joseph Jastrow, the first secretary of the American Psychological Association and its president in 1900, was appointed to a chair of psychology in the University of Wisconsin in 1888, and has occupied this position for an unbroken period of thirty-five years, a record unique in the history of our science, therefore
>
> RESOLVED, that the American Psychological Association, meeting at Madison, presents its sincere congratulations to the University of Wisconsin on the long and distinguished service rendered by Professor Jastrow to it and for the advancement of psychology.

The development of psychology during this tenure of office was so rapid and in such varied directions that those who stood in the center of the movement at times failed to sense rightly the changing perspective of interests and problems. The tendency to specialize was partly responsible, and the absorption in the immediate problems connected with the routine work of instruction interfered with the formulation of long-range plans and a critical appraisal of the relative bearing of movements from time to time inaugurated. Retrospectively the situation seems clear, and it is difficult to detach the present outlook and recover the stage-by-stage development of the several movements converging toward what may well be called the American Renaissance of Psychology.

Interpreting this in terms of my personal interests and activities, I should emphasize as the major considerations by which I was guided, first, the genetic point of view. The earliest expression of it was in my course in Comparative Psychology. When I began, the older view of animal intelligence still prevailed. I could find no more suitable text than Romanes' account of *Animal Intelligence,* full though it was of questionable anecdotes and uncertain conclusions. T. Wesley Mills at Montreal offered a more scientific survey, and with the appearance of Lloyd Morgan's *Comparative Psychology* it was at least possible to indicate the bearing of the analysis of animal behavior upon human psychology. There is no topic that so rapidly changed its character and still so well reflects the progress of modern psychology. In due course I used the contributions of Thorndike, and later those of Watson, with their emphasis on experiment and the elimination of the anthropomorphic interpretation.

The second sustaining interest lay in the contention that the study of abnormal phenomena was indispensable to the interpretation of the normal. My courses in Abnormal Psychology remained central in the general program.

This interest was strongly affected by the public attention given to the movement inaugurated in 1882 by the Society for Psychical Research. The prospect of directing this popular interest to allied problems in abnormal and social psychology attracted me. It gave me also an interest in the arts of deception on and off the conjuring stage, which led to a personal acquaintance with such leaders of the art as Herman, Keller, Houdini, Thurston, etc. The result was a volume published in 1900—*Fact and Fable in Psychology*—which seems to have played a considerable part in public enlightenment in this field.

At this and later periods hypnotic and trance states received much attention. The mechanisms involved in them must find their clue in abnormal states. The underlying concept was that of subconscious operations. It must be remembered that though Freud had published his *Studies in Hysteria,* it was not until the appearance of his *Interpretation of Dreams* (1900) that his views were at all known in this country. It was this work that impressed me and led me gradually to regard the Freudian view as an important instrument in the interpretation of the abnormal. More fundamental in my view was the analysis of subconscious phenomena within the normal life. When I published in 1906 my book on *The Subconscious* (later translated into French) I was strongly influenced by the then prominent investigations of Frederick W. H. Myers, who, however, approached personality from the possibilities of survival, but included a broad interpretation of the entire data. I was equally impressed with the phenomena of dual personality which, described at an earlier period by Azam and Binet in France, were brought to the focus of the American attention by Dr. Morton Prince. If I had presented the book ten or even five years later, I should certainly have incorporated more fully the Freudian point of view.

I had not at that period given close attention to the phases of abnormal behavior which in the last twenty years have come to dominate the field, namely, the psychoneuroses, the hysterias, the neurasthenias, the varied psychopathic states. My courses in Abnormal Psychology began to emphasize this aspect because of its bearing on the varieties of character and disposition. What is now so familiar as clinical psychology was substantially unknown.

It was about 1908 that I reached the conviction that a major clue to character and personality was offered by a study of abnormal temperaments. The incentive to formulate my views came through an invitation to deliver a course of eight public lectures at Columbia University in 1910, where I gave graduate courses in Abnormal Psychology for the semester. I chose for the subject "Character and Temperament." I surveyed the groundwork of personality in terms of the contributing factors in the total psyche. The approach led through a study of the sensibilities and the recognition of the basic rôle which emotion plays in conduct. This was part of the general reaction against the extreme intellectualism of the dominant movements of psychology; I placed the intellectual guidance as a higher form of control of urges and processes supplied by organic trends and by the emotional development. This program necessarily called attention to the rôle of individual differences, including the abnormal traits. It made a place for the group traits or social tendencies, and considered the part of the environment in shaping personality and the resulting characteristic qualities of men. In brief, it was a cross-section of the psychological field by way of the then somewhat novel integrating view of personality, a field which since then has grown to be a central interest in modern psychology. I published in essay form, with more of a literary appeal, a little volume on *Qualities of Men* in 1910, and the volume called *Character and Temperament* in the same year.

I have mentioned in an earlier connection my strong interest in the province of logic, as well as in the psychological products in terms of belief; it was the varieties of human beliefs that attracted me to the field of so-called "psychical research," as I recognized what a large part such beliefs held in the history of culture. This anthropological approach I had followed as early as 1891 when I gave the Vice-Presidential Address before the Section of Anthropology of the American Association for the Advancement of Science on the "Natural History of Analogy." This includes the field of credulity and superstition and considers the mental operations of the primitive mind. Carrying this interest through various stages, and by way of articles in the popular magazines, I assembled my contributions under the title of *Psychology of Conviction* in 1915. I included in this volume as well controversial problems in which psychology had an authentic voice, and thus illustrated by the case method the varieties of influences that affect belief and the range of current social and cultural problems in which psychology has an authentic voice.

An account of so long and active a period in psychology and of one's personal participation in it is influenced by the later perspective that gradually emerged. There is the further difficulty that the pressure of a full program of instruction and the limitations of a professor's life stand in the way of a free and favorable development of one's intellectual interests. One must keep in touch with the needs of students and the newer advances in many directions. The seminar in psychology offered the best opportunity for presenting original material and for finding in the stimulation of a body of graduate students an exchange of opinion. The material used in published books as well as the experimental studies of the laboratory were developed by means of seminar conferences and reports.

Disregarding the order of development, I shall survey the several formative concepts that most definitely influenced my activities. In the experimental field it was not possible to organize the research without reference to the ability of the students and the necessity of their completing a piece of work for thesis requirements. In the earlier researches from the Wisconsin Laboratory the problems of sensation dominated, but they included an investigation of involuntary movements, republished in *Fact and Fable in Psychology*. Other studies from the Laboratory include investigations in association, especially in terms of community of ideas, in illusions and their revelation of mental habits, an elaborate study of stereoscopic vision, contributions to mental tests, an analysis of the factors of intelligence, including their failure as instances of stupidity, the relation of focal and marginal factors in apperception, and a careful study of the judging process bearing upon measures of agreement of juries, and the order-of-merit procedure.

The advantage of presenting from year to year a considerable variety of experimental problems was that of offering to students an opportunity to follow the rapidly growing fields of research, and the methods developed for the purpose. As my own special interests turned to the broader aspects of the interpretation of mental phenomena, I gradually shared the experimental work with the laboratory assistant, and in the latter part of this period the conduct of the laboratory was in the hands of Dr. Clark L. Hull, who beginning as fellow and assistant became in due course Professor of Psychology at Wisconsin until the present year, when he transferred his activities to Yale University.

I have recently characterized the approach which seems to me most fruitful as applied to the whole range of the world of mind, the naturalistic concept. By this I mean the following of the clues of nature in determining the meaning of any mental function. This view arises from the renaissance of biology through the illumination of evolution. It equally places in the foreground the factor of growth; and it was this that led me gradually to emphasize the importance of child psychology.

The child-study movement is inspired by the general principle of applying psychological principles to the understanding and directing of child nature. My application was the converse one, namely, the bearing of child behavior upon psychological principle. Whatever is fundamental in the field of mind must appear early in child development. This idea, now so commonly accepted, has converted the nursery into an informal laboratory, awaiting the formal child research laboratory now introduced at several centers. In other words the child's repertory of reaction appears as an ancient record of primitive mental patterns. I see no reason why one should not adopt the general term "primitive psychology" to include the field of infant and child reaction as well as the mentality of races living under primitive conditions, and extend the term also to include low-level behavior within the adult human organism. This phase of anthropology is in a similar sense a record of primitive racial psychology. The result is a genetic psychology which is even at present only in the making. The growth of the mind in human development appears as a transformation of primitive psychology into adult psychology, and equally from the ways of thinking of primitive men to those of civilization. It is evident that the well-recognized shift from structural to functional psychology, from a static to a dynamic view of all mental operations, is but another expression of what I prefer to call the genetic concept. While the older study of sensation was concerned largely with determining the types of discrimination and reaction to physical forces which the senses provided, the genetic view focuses upon the part such function plays in the service of the mental life. The distinction between protopathic and epicritic functions is an issue of this concept. Mental functions, like bodily ones, arise in the evolutionary process in terms of their bearing upon the welfare of the organism. This view determines the perspective of problems. However interesting on their own account are the procedures by which the eye serves the purpose of discrimination and finding one's way, the directive problems of vision are derived from biological origins. Thus the requirements of night and day vision, the development of the color-sense, the perception of the fovea for accurate vision especially within the reach of the hand, the interpretation of the third dimension, the perception of motion—all these processes provide problems of functional proficiency rather than merely analyses by which the world of visual space is built up. In this same view appears the fact that functions primarily of biological utility may equally develop a secondary service. Thus, we have no aesthetic senses, and the aesthetic life is a by-product of visual, auditory, and other sensory processes, established for their direct utility in quite other relations. Similarly, in the field of hearing, we have such various senses as the sense of auditory direction, feeble in the human kind, the discriminative recognition of noises and tones, the sense of pitch, and the unique sense of pitch-relation which makes music possible. From the use of the ear as an auditory signal of alarm to the enjoyment of modern music is a long genetic range.

In the field of total behavior, the genetic series extends from the organic responses now so minutely studied in the autonomic and glandular regulation and in the basis thus provided for the emotional life up to the elaborate regulation of behavior by logical reflection.

This fundamental approach gradually took shape and determined the sequence of my interests. Whether considering the phenomena of the subconscious, or the child responses, or the mentality of primitive people, or the relative play of feeling and thinking in human development, I was able to find an inclusive point of view in this genetic concept, which in turn leads to a naturalistic psychology.

I shall at this juncture interrupt the account of the maturing of my general perspective of the scope and significance of psychological principles and points of view by some reference to the vicissitudes of a psychologist's career in its academic setting.

The psychologist, in common with other holders of a professional chair, may have his activities and his life-plan determined as much if not more by his academic duties and relations as by his professional interests. He may, if so disposed, withdraw as much as possible from any share in the management of his own university and from any interest in educational problems and welfare. Such withdrawal may promote concentration upon one's scientific career; it was incompatible with my interests and temperament. I contributed frequently to educational journals on a variety of academic and educational subjects.

I soon became convinced that the policies of control of the University were fundamentally wrong. Whatever minor improvements and reforms might accomplish, there seemed to me no possibility of patch-work redemption. All essential decisions were by external authorities who could have no adequate knowledge of the issues involved. The President and Deans conveyed that authority or imposed it upon the members of the Faculty, who under typical circumstances or worse, found themselves bound by limitations unsuitable to leadership and, unless resisted, leading directly to subserviency. When I was invited to participate in a discussion of the status quo in American colleges in a symposium held in connection with the inauguration of President James at the University of Illinois, I was quite unprepared to find that the same protesting point of view would be presented by Professors J. McK. Cattell of Columbia, J. P. Munroe of Boston, and others. Dr. Cattell assembled the convincing data and opinions in a volume on *University Control* (1913) to which I contributed.

I welcomed and took an active part in the formation of the American Association of University Professors, the influence of which has been most helpful in this cause. When the Carnegie Foundation for the Advancement of Teaching was instituted, I cordially endorsed its purposes, influenced its directors to include state universities in the provision, and to my regret was obliged to denounce its policies when it seemed to me as to so many others that, despite the mitigating circumstances, it was open to the charge of failure to fulfill the obligations of its important trust.

Viewing the academic situation retrospectively, I adhere steadily to my original position, and believe that the position of the professor in American universities will fall far short of its possibilities until the control of all vital university affairs is in the hands of the Faculty; that the President and Deans should have such authority as the Faculty deems proper and useful and no more.

The temper of the relation between the professor and the "authorities" has vastly improved in the more favorably situated institutions; but the mode of government has changed but little. I have believed through all these years and continue to hold that the American professor is constantly exposed to influences that seriously interfere with the development of his best powers and the content and satisfaction of his ways of living. His financial limitations are serious; the methods of securing advancement are still more subversive; the prospects of converting an academic career into an adjusted and satisfactory life are not sufficient to attract young men of proper ability to that calling. The quality of the professor has declined. The tragic part of it is two-fold: that so many professors are resigned or insensitive to their fate, and that so much of the frustration, misery, failure, and abandonment of high purposes is needless. Under the favorable circumstances of a liberal self-direction the American professor could exert a far more potent influence than he now attains, and lead a fuller and more adequate life.

This narrative would be lacking in frankness if it failed to record the large loss of time and interference with consecutive work through incapacity. In 1894, after I had carried the double responsibility of my preparation and direction of the psychological exhibit at the World's Fair and my university work, I found myself completely prostrated and was obliged to lose a year in recuperation. As there were no circumstances in my life to account for this breakdown and as the hereditary disposition was established, the main factor was overwork.

On resuming in the fall of 1895, I had to continue for years with uncertain energies; usually a three-hour session of university work would require a complete rest for the remainder of the day. The summers were spent wholly in recreation on the coast of Maine. It was there that on one occasion I found my physician called into consultation to see William James, whose program of work was even more seriously interrupted by a similar incapacity, in his case complicated by a mild organic trouble. In our interviews before and after this meeting, James and I had rarely mentioned this liability, which was, I am told, shared by both Wundt and Bergson, and thus had the compensation of good company, but we preferred to discuss psychological problems. I found much encouragement in the heroic achievements and genial personality of William James.

I decided in 1898 that it was futile to continue in this handicapped condition and arranged to go abroad for fifteen months, on the only terms then available, namely by providing a substitute. I took with me the materials for the articles I had agreed to prepare for Baldwin's *Dictionary of Philosophy and Psychology*, worked on them at Oxford for four months, and then enjoyed a year's holiday in European travel. My interest in the art centers and the historical treasures of Europe was strong and has remained so, passing somewhat beyond the amateur stage.

From 1899 on I simplified my mode of life; and for a period of years, though I had ever to husband my resources, was able to give some energy to researches and the writing of the books enumerated. The financial pressure was continuous and additions to income imperative. I began to give popular lectures and have continued this activity and enlarged it as the years went on. This was another expression of my conviction that the popularization of psychology was essential to its public appreciation and official support. I found these outside contacts stimulating and welcomed lecture tours as breaks in the continued strain of teaching. For I have always found the personal output of teaching a serious draught on energy, and it must be so unless reduced to a lifeless routine.

There is little to record but ups and downs until 1922, except the disturbance of the War period. I undertook to prepare a treatise on what might be called the "Psychology of War and Peace," to be used as a text for the Reserve Officers Training Corps in the Spring of 1919. The work was nearly ready when the armistice made it unnecessary. The years ensuing were most difficult. The cost of living rose enormously; salaries remained almost as they were. Summer vacations had to be given up.

In 1922 an organic difficulty appeared which led to a successful minor operation in December of that year, but which induced a condition of exhaustion both before and after the operation which again required a year of difficult recuperation. My recovery was slow and uncertain, and the three years that followed, ever full of anxiety and illness, remain a memory of trial and despair. I continued to conduct my university work, reducing it to a minimum, yet under circumstances likely to undermine a stronger constitution than mine.

The anxieties of this period centered about the condition of my helpmate, who shared with me the responsibilities of my entire career in Madison. I knew for years, that, despite the partial relief afforded by a serious operation, the illness was a fatal one. With her loss a return to Madison was beyond my courage. In view of the circumstances I was granted a leave of absence for the year 1926-27. I then resigned my professorship anticipating the period of retirement under the Carnegie provisions by one year.

1927-1930. Early in 1927, though incompletely recovered from years of strain, I found it necessary to formulate my plans for the future. I had always favored early retirement, looking upon this period of one's career as a change of activity rather than a cessation. Retirement might well bear another name indicating a direction of energies with a freedom from compulsion and an opportunity for expression, quite too commonly denied in the pressure of exacting duties of a professional life.

New York offered the only possibility of the varied contacts which I felt essential to support me in my several activities. I had lectured there frequently in the period between semesters, which alone I could devote to tours distant from Madison; my associations with editors and publishers were many. I was fortunate in finding that my services as a lecturer were acceptable to the New School for Social Research. Its able director, Mr. Alvin Johnson, reacting hospitably to the increasing demand for psychological guidance in social problems, had included in his program a generous provision for courses in psychology. To speak to audiences, attracted by intellectual stimulation and the opportunity of contact with progressive minds, fell in with my convictions concerning the importance of the movement called "adult education." I am convinced that the appeal of such a program, represented by the New School for Social Research, has an important future throughout the land. The confidence in its future in New York is shown by the erection of a permanent building for the venture, to be occupied in 1930.

In the fall of 1927 I inaugurated a series of syndicated daily articles under the title *Keeping Mentally Fit,* in which I give brief and simplified discussions of psychological principles and their application to daily life. The demand for guidance in mental difficulties and maladjustment was indicated by the letters which the articles evoked. It became desirable to devote half the articles to replies to readers presenting personal, at times, general problems. A selection of these articles appeared in a volume of the same title in 1928.

I am attempting to bring to completion prospects that were postponed, in several cases abandoned, by reason of the increasing pressure of academic duties and the obstructing circumstances, personal and professional, that seem quite too commonly attendant upon the type of career to which I was committed. Utilizing my accumulated material, I assembled it in part for my courses of lectures in New York and have in preparation a volume thus derived on *The Life of the Mind: Thinking and Feeling,* and another on *The Quest of Fate: Psychic Cults and Systems.* I likewise prepared a systematic selection from my recent daily articles to make a manual of guidance in what in the broadest sense becomes an aspect of mental hygiene under the title *Piloting Your Life: The Psychologist as Helmsman.*

Except in my fallow years, I have contributed at frequent intervals articles to the popular magazines. Much of the material on *Fact and Fable in Psychology* and in *The Psychology of Conviction* first appeared in article form. The demand for such articles is an expression of the widely extended interest in psychology. I am assembling a collection of articles in a volume entitled *As the Psychologist Sees It.* It has been my practice to review current books in psychology for various periodicals. Though the task of reviewing from a score to two score books annually is at times taxing, it forms an effective stimulus to keep in touch with the encyclopaedic sweep of modern psychology.

As I have indicated, it is impossible to state definitely and at what stages and under what stimulations, I matured my fundamental positions in psychology and in following them undertook one and another line of research and interpretation. Fundamental is the objective study of behavior under the direction of the naturalistic point of view. This approach has become familiar under the title of behaviorism, a term appropriate in its general bearing and under a liberal interpretation, but unfortunately associated too commonly with a narrow and extreme formulation. With the establishment of an objective and an experimental point of view and of the underlying biological approach, all psychologists became behaviorists; but what they included under this term and the methods which they adopted to promote their contributions became the decisive issues.

When, in 1914, Professor John B. Watson published his book on *Behavior: An Introduction to Comparative Psychology,* I at once adopted it as a text for my course in that subject. Though I could not fully endorse his claim that the whole of psychology could be pursued by the method he so well applied to the "primitive" objective phases of response, I found the statement that psychology must proceed as a "natural science" with the emphasis on (functional) behavior wholly in accord with my own formulation. But with the appearance ten years later of a set of popular lectures on *Behaviorism,* his claims advanced to such presumptuous and fallacious expression, that I was compelled to recognize the menace to psychology inherent in this extraordinary set of destructive and constructive conclusions, based as they were, on data remote, misinterpreted, and wholly inadequate. The amount of ignoring and discarding of facts and interpretations quite as scientifically established as his own confident assertions was staggering, and the change from "Behavior" to "Behaviorism" seems to have transformed a promising science into a confusing cult.

Watson's publications in the five years that followed confirmed my distrust all too plainly. The more generally the term behaviorism is used, implying in Watson's treatment extravagant pronouncements, the less do I see the need of its use, since substantially all psychologists have become behaviorists by reason of the objective and naturalistic trends that permeate modern psychology in so many (not all) of its expressions. I appreciate the significance of objective work in primitive psychology, such as that of Lashley, as a model of scientific procedure. As a naturalistic psychologist I must be a behaviorist; and I agree that the fundamental interpretations and methods of approach and investigation must be found in the study of primitive psychology; but to restrict the total range of psychology to the primitive patterns of response is to deny the genetic principle, instead of tracing it in its ascending development. It is in the misleading of the lay interest in psychology and in the discrediting of psychology in more critical circles that the Watsonian pronouncements, but for their limited and declining influence, are likely to retard and mislead the development of psychology.

In terms of my present and definite interests, the advances of the last twenty years seem far more vital and significant than those of the twenty years before, though my retrospective view recognizes the indispensable stages of progress of that earlier period. But the false leads, academic inertias as well as enthusiasm, seem to overshadow the solid and enduring contributions. The American renaissance of psychology is substantially an achievement of the last two, possibly the last three decades. Psychology as we know it, along with many another discipline, is a twentieth-century achievement.

It is just twenty years ago that Freud and Jung accepted the invitation of Clark University to present their views to American psychologists. However hesitant or reserved one's acceptance of what must be designated the Freudian contribution, the revivifying effect of that movement must be gratefully acknowledged. I have recorded elsewhere my perspective of significance and measure of acceptance of the Freudian position, as well as of the menace which I recognize in its cruder, doctrinaire, and extreme application. The Freudian era forms an important component of the psychological renaissance of today. The correction and expansion of the concept of the urges and their issues in complexes under the manifold stresses of modern life, including the contributions of Jung and of Adler, have gained for the movement a wide adherence. The company of out-and-out Freudians on the original program, which Freud himself has considerably modified, is decreasing. Psychology will never return to a pre-Freudian status.

It is in my opinion unfortunate that the fate of the Freudian psychology was so closely dependent upon the acceptance of the psychoanalytic technique in the treatment of the psychoneuroses. That to me is but one application—doubtless a most important one—of the set of principles whose major significance lies in their illumination of the motives of behavior, normal and abnormal, and the components of personality, including deviating and unbalanced characters. The system illuminates as well the psychic products and deposits of cultural expression (myths, customs, social structures); and offers a guide to the wise development of childhood and the ideals of sanity (mental hygiene).

These benefits remain even if psychoanalysis proves to be a procedure of limited applicability often fraught with danger. The detailed development of the Freudian plot in the family romance and in the sexualization of the total psychic energies may fall away entirely, and Oedipus retire to his classic seclusion, and yet the Freudian interpretation of the drama of life, when sanely staged, retains its significance. In the field of treatment psychoanalysis will be but one of the many instruments—to be discriminatingly employed—in the beneficent kit of the psychotherapeutist. Psychiatry must remain a far more inclusive discipline than psychoanalysis. Yet it was largely—by no means wholly—through the revivifying theories of Freud that a psychological psychiatry has made its way. The "new" psychology is often applied to the Freudian renaissance.

The emphasis on emotional development, the concentration on personality problems, the supreme significance of childhood experiences, the close relations of the processes of normal adjustment to the liabilities of maladjustment through abnormal handicaps and deviations: these were in a measure already slated on the program of the "new" psychology; but the impetus to their inclusion and the manner of their formation were profoundly affected by the constellation of principles that will remain as closely associated with the pioneering contributions of Freud, as is evolution with the work of Darwin.

Similarly I find an acceptable correction of the nuclear problems of experimental psychology as dominated by the physiological approach in the Gestalt psychology. It corrects the too rigidly analytic interpretation (in the older intellectualist sense) by a functional synthetic view of the psychic life and its supporting processes. It reinstates the naturalistic totality of each response pattern in the total situation.

To speak of the renaissance of psychology, especially in the American setting, without explicit recognition of the practical motive would be a glaring omission; for that renaissance found its momentum in the appeal to psychology for the regulation of human affairs. Applied psychology is in many a quarter the pay-vein that supports the mine. The educational application is the oldest and most comprehensive; mind training is *the* psychological cult. Yet educational psychology has broadened beyond the investigation of the processes involved in the curriculum to a truly psychological view of the developmental procedure of education. The application to industry in the interests of efficiency and the selection of personnel have brought about a psychotechnic movement which has crossed the seas and added to the European repute of American psychology. Less helpful has been the reputation if not the notoriety of advertising as an American specialty. The psychology of advertising has itself been advertised beyond its intrinsic importance. Psychological interest and commercial interest must not be confused.

All this is a far cry from the academic "purity" of the experimental laboratory in its inception. Despite the tendency for the pressure of application to overshadow the patient investigation of principles, no one regrets the impetus to psychological pursuits fostered by the rewards of profitable, if at times hasty, application. In the future the psychotechnician has a secure place.

My own interests have been on the side of the study of proficiencies and deficiencies as they play their versatile parts in the development of character as well as career. Aptitudes and traits are closely interrelated; in this view the social and clinical applications dominate. The important problems of delinquency and crime thus acquire their modern phases. The restatement of the entire view of criminality must be credited to the applications of psychology. Courts and procedures, sentences and treatments must continue to follow this lead far beyond present provisions..

The part played in this movement by mental tests is familiar. My interest in the subject goes back to 1893 and before. In that early period Cattell had emphasized the importance of tests as indices of individual differences. The range of human capacity is important independently of its application in serving as a measure for placing the individual in a scale of performance. It is the correlation of traits thus determinable that affords a means of gauging what facilities and aptitudes go together. The bearing of such data upon the problems of heredity, by using resemblance as in index of common inheritance, added to its value. But it remained for Binet, whom I had visited in Paris, to recognize in ordinary achievements (not merely in specially arranged sensory, motor, memory, and intelligence functions, such as I had used) an available means of grading natural aptitudes. Intelligence testing has become a technical specialty; my interest was in its bearing on the nature of intelligence and the range of individual differences.

While the practical value of tests in educational, industrial, and clinical diagnosis is responsible for acceptance of the *I.Q.* as a gauge of mental ability, the earlier problems concerning individual differences still remain.

Social psychology has been an effective means of popularizing and capitalizing lay interests; much of it is of uncertain bearing and questionable value. The sociologist as well as the educationalist has borrowed indiscriminately from the psychologist in the attempt to make a scientific specialty of what is for the most part a compilation of generalities reënforced by experience. Psychological sociology cannot replace the specific psychology of the social relations upon which the more expert handling of the human social individual depends as well as the understanding of psychological forces expressed group wise, in crowds, movements, fashions, cults, cultural products generally. The social psychologist of the future has a responsible task.

Yet of all the applications, that of clinical psychology appears to me the most momentous. The alliance of psychology and medicine—which has an ancient though casual history—is significant and with psychiatry most so in the modern development. The ministering to a mind diseased has assumed the broader ministration to a personality maladjusted and to souls in distress. Personal, family, social situation serve as clues to the formation of a neurosis, the symptoms of which, however actively they remain the actual impediments to normal aptitudes, form but part of the broader case-history. The overwhelming prevalence of minor psychic incapacity in the modern ways of living has compelled attention to what has come to be a major problem of social welfare.

Our provisions for it are wholly inadequate. Despite the increasing facilities of neurological institutes and psychiatric clinics in close affiliation with medical centers, the need for a service specifically adapted to the situations of the psychically impeded and handicapped is imperfectly organized. The struggle to make a living and lead a useful, satisfying life is an imperative problem, one of first magnitude in the social welfare of the present and future; its urgency has been impressed upon me by the hundreds of letters from the army of maladjusted, responding to my suggestions in the interests of mental health, and by the disheartening procession of patients helpless to find the aid they require. Clinical psychology is ready for an expansion on an unprecedented scale. It cannot proceed upon the initiative of some man of genius, such as the surgical organization of the Mayos, much as it needs such inspiration, for it requires the home treatment of the individual in the life situation. Like the Freudian attempt, it must explore the most vital, defensively guarded sanctum of the personality, and operate upon the intimate relations that make life worth living, or fail to do so. The clinical psychologists of the future must be drafted from the ablest ranks of men and women, whose expert training is combined with a flair for wise living.

The direction of this movement, by way of prevention and by establishing right norms and patterns of behavior, has been organized as *mental hygiene.* It is obvious that the movements—clinical psychology and mental hygiene—have a common aim, and must proceed upon the same insight. Mental health, moral poise, social efficiency are the aims; and the foundation of these in what, for lack of a better term, may be called the neurological concept of behavior sounds the modern note of guidance and control. The art of human guidance centers in the many-sided insights of the psychology of personality.

I can find no peculiarly pertinent place to refer to that parallel interest in psychological phenomena organized as psychical research; so I shall insert it here. In this field also there has been a succession of innovations, notably that prompted by the discovery of ectoplasm, but little sign of scientific advance apart from a more critical examination of claims and a tendency to dissociate the conclusions from a spiritistic interpretation.

The problems of psychical research engaged my active interest from the beginnings of that movement, which in the closing decades of the nineteenth century was so prominent that in many circles a psychologist meant a "spook hunter." I have made it plain in several essays that the interests thus compositely referred to are of wholly different status both logically and psychologically. The first is the embodiment of folklore beliefs and their survivals and revivals. In this the belief in spirit and human survival after death occupies the chief place. Next is the belief in powers transcending those acknowledged by psychology. Third is the occurrence of abnormal mental states in which such powers are exhibited. Fourth is the story of deception by which belief in transcendent powers is maintained. From these, other interests radiate. The scientific interest centers about telepathy with its possible experimental confirmation. The popular interest goes out to mediumistic revelations. The phenomena offered in evidence (apart from revelations, premonitions, dreams, apparitions) fall into the physical appearances, typically those of the seance chamber and the mental disclosure of private affairs.

I have firmly held to the opinion that all the physical phenomena without exception were fraudulent and with few exceptions deliberately so. The "psychical" phenomena stand on a different basis. I am unable to accept the proof of telepathy and so cannot use that hypothesis in the further explanation of baffling evidence critically reported. Trance states are real, and abnormal exercise of sensory and other powers occur in them. The number of well-authenticated instances, amid the myriad that are not so, that must be left unexplained is considerable. One may be open to the charge of dogmatism in affirming that there are no modes of communication, no forms of mental operation, except those definitely recognized in psychology, and in concluding that the entire range of evidence may be dismissed as presumably the issue of coincidence, faulty observation, prejudice, etc. Yet such a conclusion seems to me more probable and more profitable than any other. I can find no (biological) possibility for the evolution of such powers, cannot conjecture how they could have developed in the human psyche, how they became disused, how they sporadically and rarely appear. The case against "psychical belief" (as presented in the volume published by Clark University, 1927) seems to me definitely stronger in every respect than that for it.

Once admitting the favorable possibility, it is difficult not to follow it along the road which leads to so extravagant a position as that of Richet, eminent man of science though he be, who erects a system of "metapsychics" defying or transcending established laws of psychology, biology, and physics, indeed disintegrating the world in which we lead our practical as well as speculative lives. The three beliefs which he exalts to the principia of Metapsychics are a power to transcend bodily senses, to move objects at a distance without mechanical agency, to form ectoplasm. "C'est le premier pas qui coute." If this is the logical issue of "psychical research," I do not see how either a loyal psychologist or physicist or biologist can assent to the first step.

Yet in the course of investigating this varied and engaging series of phenomena, psychology has found enrichment and enlightenment of problems lying in unexplored frontiers of the mental domain. The psychology of mediumship is a legitimate subject of investigation; so likewise both as psychology and as anthropology is the significant story of human beliefs in superhuman powers. This story belongs to the treasured records of how the human mind learned to think, by what interests it was moved to develop beliefs as to the nature of the universe and man's destiny in it. It is at once an anthropological and a psychological deposit in the evolution of culture. I propose to treat it as such in its several ramifications, in a volume called *The Quest of Fate: Psychic Cults and Systems.*

I have found it simpler to present my points of view in psychology and the dominant interests which have emerged with the passing of the years, by way of an eclectic statement of opinion, retrospective and prospective. So far as circumstances have permitted, I have attempted to contribute to one and another of a group of interests, not detached and unrelated, yet indicative of a desire to see psychology soundly and to see it whole. There will ever be in all the sciences, and in psychology above all there should be, a group of foresters concerned with the general contours, as well as a larger group of tree specialists upon whose minute labors the growth of the science depends.

Psychology is under the temptation of attempting too ambitiously to be all things to all men. Having become a word to conjure with, it has been peculiarly open—indeed has always been so—to abuse of unscrupulous practitioners. The pseudo-psychologists, preying upon a public eager but uncritical, have found their opportunity in the very loyalty of responsible psychologists to sound scientific advance. On the book counters recipes for success, cure-alls for ills, formulae for character-reading, rules for prediction of fate, frothy inspirational appeals to master handicaps, to develop latent powers, and to ignore reality by a buoyant faith, vie with reputable attempts to bring psychological doctrine to the masses. The one leads back to the jungles from which the folk-mind has never completely emerged; the other lays out the ways and plots the course for a wiser development of such powers as each possesses and a calm acceptance of their limitations. The future of psychology as a contributor, along with many other disciplines, to human welfare is assured. But the progress cannot be easy; nor can the counteractions of false pretenses be omitted from the constructive program. The responsible psychologist will accept the ideal of a scientifically minded humanitarianism.

These surveys of the careers of American psychologists were expected to include a comment upon the prospects of psychology as they may affect those contemplating that profession. Psychology is riding upon the crest of a wave of rapid expansion. The time for retrospect as well as prospect is appropriate; the year 1929 marks the jubilee of the foundation of the first laboratory of psychology. That event was appropriately observed by the holding of the first International Congress of Psychology in America. I may have been the only American member who attended both the first Congress in Paris in 1889, and the last in New Haven in 1929.

At New Haven I considered the "Conflict of Psychologies" (*Sci. Mo.,* 1929), a conflict in many respects more apparent than real, at the skirmish lines rather than in the basic positions. Yet there is an unfortunate disposition in this camp and that to claim the entire field for one school, one method, one concept. Of this the extreme behaviorist and the extreme Freudian offer conspicuous examples. As disciples gain independence, these contentions, I am convinced, will recede; for they are out of touch with a progressive program. The tendency is toward the integration of the concept of mental behavior in a broadly inclusive view of the evolutionary ranges of mind. The growing use of such terms as "gradients," "emergent evolution," and the complication of action patterns, confirms the conviction that the mechanisms and motives of behavior conform to no one formula; certainly the direction of human behavior can proceed profitably by no one cult. I have adopted the term naturalistic psychology for my own inclusive view.

These positions have an intimate bearing upon the future of psychology and upon the careers of those who are to share and direct its fate. The era of specialism is established. The psychologist dealing with primitive types of behavior will have one intensive training, one dominant interest, one order of expertness. He may find his material in animal psychology or in child psychology, disciplines in turn demanding different qualifications. He may be a specialist in the psychology of infancy or of adolescence—domains psychologically worlds apart. If a student of the abnormal, his temper, his flair, his approach, and grasp are of another order, as his orbit of application is similarly distinctive. To conclude that the clues to human behavior all lie in the animal maze, or in the nursery, or in the clinic, would be equally misleading, equally hampering to the progress alike of the specialized field and its contributions to the total advance.

The psychologist of today faces the danger of too early and too close specialization. Because the older régime was alike strong in generalization and weak in scientific method, that association is likely to leave a false "conditioning" impression. The integration of modern psychology demands a broad foundation, as equally it calls for the expertness of the specialist.

Avoiding these dangers, the psychologist of the future will do well to acquire a historical sense of his antecedents, such as is presented in the recent work of Gardner Murphy. He should be aware of his inheritance and appreciate his advantage in approaching as clearings what to the pioneers of a generation or two back were thickets with hardly more than an imperfectly blazed trail. He can plan his problems in a setting already provided. His is the danger, however, of staking too much on an innovation or a refinement of detail, and of discarding the more significant if less exact generalizations of a more penetrating aperçu. There is likewise the danger of overattention to application and the temptation to have one's problems set by practical needs, to the neglect of the fundamental, often theoretical analyses from which the fruitful problems derive. Certainly in terms of interest and convergence of insights upon understanding and guidance of human affairs, the young psychologist enters the calling at a propitious era. The foundation of an Institute of Human Relations is a timely expression of this convergence of interest. The scale of its provisions indicates the scope of the equipment which scientific invention is prepared to provide. The psychological laboratory is no longer to be relegated to unused quarters or outgrown habitations of better established sciences. And by no means least, the psychologist's services, when he can offer them with the confidence of professional training backed by natural powers, will be in demand.

In one respect, as indicated, do I view the prospect with misgiving. Most psychologists will be mainly or mostly professors; and the present status of the academic pressure is not conducive to the freest development of the outlook and the contacts and the self-direction which are peculiarly essential for the psychologist. In addition to the economic and the administrative there is a psychological maladjustment in the academic life which must be relieved if men of learning are to develop their potential powers.

With the assurance that psychology is established in modern life, and with the varieties of its application in the modern world, the young psychologist can take a long-range view of the prospects of his profession. That prospect as seen through a long range retrospective vista is one of promise and significance. It may be that poets are born in a sense in which engineers are not; but every specialized career is the reaction of a temperament to the opportunities and stimulations of a cultural environment. The favorable temperament for the psychologist—now that his opportunity has arrived—is a composite of a flair for human relations and a comprehensive analytic insight into the sources of human behavior. The proper study of psychological mankind is man.

F. KIESOW*

The first incentives to my psychological pursuits were certain experiences of a didactic and educational sort. Long illness, and afterwards home conditions, frustrated my original, ardently cherished hope of devoting my life to academic studies; for many years I was obliged to gain a livelihood by public and private teaching. After a new illness had set me back once more, I followed the advice of my doctors, and spent long years in the country, as tutor in two titled families of my native district (Mecklenburg-Schwerin). Here I had special occasion to discover that pedagogical program without a thorough knowledge of psychology is impossible. The individual differences of talents, interests, and moral traits in the several children entrusted to my care convinced me more and more of this necessity. And even at that time I was beginning to formulate certain notions, which remained mere suppositions until many years later, when I found them corroborated by the basic researches of Gregor Mendel. I studied pedagogical works to the best of my ability, especially those of Herbart, who had reached his fundamental educational views under very similar circumstances. Meanwhile I had heard, through some of my acquaintances, about the efforts of Wilhelm Wundt, who at that time was already working in Leipzig, where he had recently founded a psychological institute. He was represented to me as a man from whom I might expect sympathetic understanding. So, feeling that my long career as tutor must now end, and my health having improved, I was drawn to Leipzig by the hope of devoting myself to the study of psychology under the guidance of Wundt.

It was upon a spring morning of the year 1891 that I entered Leipzig, full of courage and hope, trusting to Destiny, which, I felt sure, had guided me in the right direction. As lectures at the University had not yet begun, I went to call on Professor Wundt, to gain his initial advice on my prospective activities. He received me kindly and courteously, and advised me to register at once for the course in psychology which he was about to deliver during the coming summer semester, as well as to listen to the elementary course then given, once a week, by Dr. Külpe; also to browse around a little in his institute, where the library would be at my disposal, and where I could take part in certain investigations in the capacity of subject for experiment.

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*Submitted in German and translated for the Clark University Press by Mrs. Susanne Langer.

The impression which Wundt's personality had made upon me during this quiet interview in his study was enhanced the first time I heard him lecture. A new world seemed to open before me. Wundt conducted his lectures four times a week, from five to six in the afternoon, in the big auditorium of the old *Konviktgebäude,* where his Institute, too, was then located. The impression I carried away from this lecture, to which I had listened in company with an audience gathered from all parts of Germany and many foreign lands, crowding the large room, was so vivid that to this day, almost forty years later, I can live it all over again. No rhetorical figures embellished the lecturer's words, no importunate gestures accompanied them. Simply and calmly, yet never monotonously, flowed his speech, fascinating by ever new ideas, delivered in Wundt's finely characteristic syntactical structure—fact linked itself to fact, problem to problem. To put it briefly: I stood entranced by an experience that was decisive for my whole life. I cannot remember having ever met with another such experience in my whole scientific career. It seems to me that the spell still binds me as it bound me then to such an extent that I never missed a single lecture. Wundt's powerful mind, with its profound insight into the vast multiplicity of all mental processes, still influences me, although I am no longer able to follow him in certain matters of detail. Wundt's basic conception of mind, of its varied individual as well as collective manifestations, which he had attained in close conflict with metaphysical and materialistic speculations, cannot be affected by mere special data, revealed by the new methodology to which the progress of a positive science like psychology is due. He always repudiated any conclusions reached by the deductive method. According to Wundt, empirical psychology is concerned neither with speculations as to the nature, location, or future fate of the soul, nor with the deductions that may be found in materialist writings of all times, but solely with mental life itself, as it is empirically given, as it is presented to us in the actual human or animal individual or in the organization of several individuals as a group. No matter how far one may deviate from Wundt in certain minor matters, I am convinced that his basic views are among those attainments of human genius which can never be lost to us again, even though they may at times be misunderstood or even despised as being antiquated.

My joy in going to Leipzig to study psychology and pedagogy was intensified because two other desires of my youth were thus destined to be gratified. Born and brought up, as I was, in a strictly Lutheran evangelical family, I had given a great deal of attention to Bible study, and had read the works of many an eminent theologian. Of these it was especially Franz Delitzsch who had made me long to read the books of the Old Testament in their original form. Thus with the help of a minister of my acquaintance as well as of a few friends of my own age, I devoted myself to the study of the Hebrew tongue, with youthful enthusiasm, and in a relatively short time had sufficiently conquered its difficulties as to be able to read the easier books of the Scriptures, with comparative ease. Encouraged, I learned whole passages of Hebrew texts by heart. At the same time I associated with a Jewish Rabbi, and took every opportunity to visit the Jewish synagogues, listening with deep interest to the Torah readings, which took me back in spirit to days when, it is reported, Jesus Himself worshipped and taught in the synagogue. It was ever the goal of my researches to reconstruct as nearly as possible the time of His earthly activity. This epoch in my life was one of deepest immersion in the personality of the Savior and in His teachings, which for me were—and still are—concentrated especially in His Sermon on the Mount. I felt deeply at times that, had I lived in His day, I would have been among His followers. To this day I experience such moments—a streak of mysticism which has survived my youth and which I derived originally from my mother. The yearning to understand the historical personality of Jesus and the scientifically established portions of His teachings impels me again and again, even now, to turn to the comparative studies in exegesis. In short, it is clear that religious motives, also, drew me toward Leipzig. Although the venerable Delitzsch, whose works had so deeply impressed me, had died before my sojourn there, I trusted that his spirit would still be felt. Besides, the University of Leipzig had other great theologians.

My second wish concerned the city's great musical resources. Because I had evinced some musical talent, which again I inherited from my mother, my parents had furnished me with some instruction both in piano and violin playing, in my little native town; and while I had attended the higher schools of Schwerin this musical education had continued. Here I had also received excellent singing-lessons from Professor Otto Kade, who directed the church choir to which I belonged. In this way I became early acquainted with the masterpieces of sacred music which still delight me. As I had later taken up my musical studies again, especially piano, Leipzig was bound to offer a particular attraction. Here I afterwards joined the mixed chorus directed by Professor Müller, which regularly performed the great German oratorios, and thus gave me a chance to become thoroughly familiar with these classics through actual participation. I will not try to speak of all that the opera and the dramatic stage offered in those days; but I would like to say with gratitude that students were enabled to visit the theater by the great boon of a price-reduction on their tickets. In this way every zealous student was given ample opportunity to acquaint himself with musical and literary treasurers of his own and other countries through actual representation. I did not let this opportunity slip.

During my first semester at the University, I attended theological, pedagogical, and psychological courses, as well as the introductory course by Külpe and another on the main problems of philosophy. Furthermore, I visited Wundt's Institute. Here I soon came into closer contact with Oswald Külpe and with August Kirschmann, at that time Wundt's assistants, and also met Ernst Meumann, E. B. Titchener, Hugo Eckener, Raoul Richter, and many other talented young men who were to make their mark in after years. Hugo Eckener, who received his Ph.D. degree on the strength of a dissertation *"Die Schwankungen der Auffassung minimaler Sinnesreize,"* (*Philosophischen Studien,* Volume 8, p. 343) based on experiments performed in Wundt's Laboratory, and afterwards departed from the Institute, disappeared completely from my life until after the World War, when I read of the success which has immortalized his name for posterity; he piloted the first airship, in only a few days' time from Germany across the Atlantic.

Of the other members of Wundt's Institute whom I have mentioned, O. Külpe, E. B. Titchener, and E. Meumann are no longer among the living, whereas A. Kirschmann is still working, with unabated powers, at the present Institute of Psychology at Leipzig. Kirschmann's wide knowledge of the entire field of optics, upon which he is now a recognized authority, was a marvel even in the old days to his fellow-members at the Institute. Thanks to the willingness and kindness with which he was always ready to render any possible help to beginners, I was able to learn a great deal from him. There was really not a question in optics to which one could not receive an exhaustive answer from Kirschmann. Anyone who has ever been in the position of a beginner, unable as yet to form independent judgments on the topic which was then the object of liveliest discussion, can form an estimate of the value of such assistance.

Of all the personalities I have met in the psychological field, Oswald Külpe is the man to whom, immediately after Wilhelm Wundt, I feel indebted for my education. At the time of my admission to the Institute, Külpe was already an instructor (*Privatdozent*) but still completely shared Wundt's fundamental outlook, as his writings of that period testify. The opposition which he afterwards offered to Wundt's theories had not yet developed. Even his introductory course was of the greatest value to me, who had never so much as heard of experimental psychology. His dignified and yet benevolent presence was—as one of my still surviving friends of those days used to say—"the kind mother of Wundt's Institute." Anyone who encountered difficulties which only an experienced psychologist could remove turned to him. Through Külpe's agency, Wundt would often hear about such difficulties, and, if by chance we heard the resultant discussion, we were sure to receive a good deal of new information. My long contact with Külpe led to a more and more intimate relation, a friendship which continued without interruption to the day of his untimely death. Külpe was animated by religious interests much like my own; he was an excellent pianist, and a great art-lover. We visited many a theater together, as well as symphony concerts and recitals. Külpe died during the World War, in July of 1915, and was deeply regretted by all those who had been in any sort of close contact with him in his lifetime.

At Wundt's Institute, one of the first things I did was to act as psychological subject for some experiments, which Ernst Meumann, who had already taken his Ph.D., was conducting, concerning the conciousness of time. This was the first experimental work in which I was able actually to participate. The problem of time commanded general interest just then, and its investigation could not have been entrusted to better hands than Meumann's. He was an exceptionally skillful experimenter, from whom a great deal could be learned; moreover, he was meticulously conscientious, and gifted with keen observational powers. The articles recording these experiments appeared in Wundt's *Philosophische Studien* (Volume 8, p. 431, Volume 9, p. 264), and are of permanent value. Another of his well-known contributions is the psychological chronometer which he constructed, and which he used for his experiments, instead of the older and less reliable instruments. Meumann's was another of the names which were later to become famous through great erudite works. The last time I saw him was at the International Congress on Phonetics at Hamburg; after that we still had occasion to exchange a few letters, then I heard that this gifted scholar, too, had been taken away by an early death. An attempt to have his *Abriss der experimentellen Pädagogik* translated into Italian failed because no publisher could be found for it.

My first semester at the University was destined to have a tragic ending. Before its close I was obliged to leave Leipzig, called home by the death of my mother. After the profound impression a man receives through the death of a mother to whom he is infinitely indebted, he cannot be the same as before. Those impressions remain in one's memory and influence all the rest of one's life. I felt a great need for rest, and spent some time on the estate of my sister, who had married a forester and lived amid the extensive woodlands of my own district. Here I became engrossed in Wundt's *Grundzüge der physiologischen Psychologie,* which had then just appeared in third edition, as well as some other writings; at the beginning of the next winter semester I returned to Leipzig.

The depression which possessed me after the death of my mother was somewhat alleviated by association with the chief pastor of St. John's church at Leipzig, whose sermons, too, I regularly attended. At the same time, however, I was attracted by the simple meetings of an American congregation, which were held during the late afternoon every Sunday in a large school auditorium. Here I came to realize, for the first time in my life, what power may emanate from a sect which clings to the early Christian tradition of extreme simplicity in all external forms.

At the University I continued to attend lectures on theology, pedagogy, philosophy, and psychology. The psychology course was given during the winter semester by Oswald Külpe, while Wundt himself gave philosophy. Wundt had arranged to have psychology given every semester. If he himself did not undertake it, it was taken over by Külpe.

In the Institute, I took part this year not only in the researches of Meumann, mentioned above, but also in the very extensive experimental work in aesthetics which Lightner Witmer was then conducting. By these investigations, too, I gained a new insight into the great realm of our science. Witmer's work appeared in the ninth volume of the *Philosophische Studien,* and is to this day one of the most valuable contributions to the field of experimental aesthetics.

Before the beginning of the summer semester of 1892, Professor Wundt had asked me to act as his *famulus,* which post I gratefully accepted. In the capacity of *famulus,* relations with the professor were particularly close the *famulus* acting as a sort of intermediary between him and his students. The institution is an old one. It goes back to the time when professors and students came to Leipzig from Prague to found a new university (1409). It had certain financial advantages, too. Besides being enabled to attend the professor's lectures free of charge, the *famulus* received from each student a certain fee known as his "chair money," which, in view of the hundreds of students who attended Wundt's lectures, mounted up to a fair sum, as things went in those days. "Chair money" is an expression which is almost incomprehensible under modern university conditions. It refers to the time when professors delivered their lectures in their own homes, and it was the business of the *famulus* to find a chair for each student. There were other little financial profits, too. The chief advantage which derived from this position, however, was the special relation into which it brought me with the Institute, the daily intercourse with Wundt himself, as well as the friendly reception which I found in his family.

The Institute in my day was open to students only on week-day afternoons, an arrangement which Wundt had made in order to leave his assistants free all morning to pursue their own researches or attend lectures. In the capacity of his *famulus,* it became my duty to open and lock up the Institute, to take care of the library and the instrument cases, to prepare the exhibits for his lectures, and occasionally to aid other members of the Institute in setting up their apparatus. I need not mention in detail all the advantages which accrued to me from these various duties. As entrance to the Institute in the morning was not denied the officers, I spent many an hour there in order to acquaint myself with all the ins and outs of the place. Likewise I furthered my own education by the use of the library, although this had then nothing like the proportions of the present library of the Leipzig Institute.

It is equally unnecessary to rehearse all the benefits I derived from my position through the daily contact with Wundt himself. One simply could not be in the presence of this man for more than a few minutes without having gained something from him. My closer access to his family was a real joy to me. Wundt made a practice of gathering about his board for Sunday dinner some of the students as well as his Institute officers. Here he was a different man from what we knew him to be in the Institute and on the lecture platform: not the rigorously stern scholar and serious teacher, but a genial head of the family, who knew how to entertain his guests with cheerful conversation. Here we could learn at first hand about his past experiences, about the relations he had held to other great scholars, could hear his opinions of various important events of the day, discoveries, etc., till the hours we spent at his dinner-table and afterwards over our coffee-cups seemed to pass like so many minutes. And I must not fail to mention the kindness and grace which Frau Wundt always bestowed upon the whole environment. This excellent lady, admired by all who came in contact with her, lent to her home a serenity that seemed to emanate from another world. She died in the spring of 1912. I had known of her illness, and received the news of her passing at the Psychological Congress at Berlin, in time to reach Leipzig for the cremation ceremonies at the South Cemetery. Dr. Mehlhorn, the well-known theologian of the reformed church at Leipzig, delivered the funeral sermon. After forty happy married years, the breach made by her death in the great man's life could never be completely healed even by the love and care with which his only daughter and collaborator, Eleonore Wundt, henceforth guided his widowed household. I saw Wilhelm Wundt for the last time at his summer residence at Heidelberg, where he had invited me and my wife to stop on our way to the Congress at Göttingen. He himself died at the end of August, 1920, at his villa at Grossbothen, near Leipzig, eight years after the death of his wife, and in the beginning of his 89th year.

Wilhelm Wundt had the satisfaction, rarely allotted to man, really to complete his life's work. He had given the world all that he had to give, and accomplished as much as a man can accomplish. He left behind him a life-work to which the remotest future generations will look with reverence, as to a monument of a great age. The reverence with which the personality of this great master and founder of our science inspired me is reflected in my present relations to his children, the daughter of whom I have spoken, who is still working in the Institute created by her father, and his son, Prof. Dr. Max Wundt, at Jena.

My position as *famulus* in Wundt's Institute was a determining factor in my career. The insight which I gained here concerning the new psychological method had convinced me more and more that a successful treatment of many psychological problems presupposed mathematico-physical as well as medical knowledge. During this semester I had attended some more theological lectures as well as a course by Professor Masius on the history of education, and besides my psychology courses I had also registered for some work in philology. In the following winter semester, however, I changed the whole course of my studies, in accordance with my new conviction. I continued to attend the philosophy course and even some lectures on philology, and listened to the second half of Masius' history of education, but most of all I felt drawn toward Rudolf Leuckart, whose extremely popular lectures on *comparative anatomy* opened new worlds again before my eyes. The theological studies I now gave up completely, having thereby learned what I desired to know. The education course was interrupted by the death of Professor Masius. Although I had profited by the course, the study of pedagogy, as it was then conducted at Leipzig, did not offer me what I was personally in search of. In this field, Ernst Meumann blazed new trails later by his experimental treatment of pedagogical problems. My studies in philology had been inspired partly through the enthusiasm of a brother of mine, who had chosen this subject for his special field and was then also studying at Leipzig, but chiefly, by Wundt indirectly, for he was then writing that part of his *Völkerpsychologie* which deals with language, and often imparted some of his findings. In this respect I was benefited especially by the lectures of Professor Sievers on *German grammar,* Dr. Hirth's Gothic exercises, and Professor Wülker's exercises and lectures on English literature and language, as well as by some other scholars.

Professor Leuckart's lectures interested me so greatly that I registered for them for the two succeeding terms, and also joined the Zoötomic Institute which he directed, in order to spend my forenoons here in practical researches. To this day I remember with pleasure the happy hours which I passed in Leuckart's Institute. Besides the material for macroscopic work which it put at my disposal, there was always a microscope available, too, for the examination of the separate tissues. Moreover, the microscope revealed to me the whole world of lower animal life, which up to that time had been perfectly unknown to me. Microscopic examinations of this latter sort held me at times entranced for hours. Here I found in a drop of water psychic manifestations that inspired the profoundest thought.

Professor Leuckart was very kindly disposed toward me and tried to further my interests in every way. Almost daily he would come before or immediately after his lectures and sit down by my side for a little while to inspect the specimens I had prepared, which usually elicited a favorable comment. On such occasions he repeatedly expressed the wish that I should devote myself entirely to comparative anatomy, and should spend my afternoons as well as the mornings at his Institute. I, however, had already made up my mind quite definitely to the study of psychology, and my afternoons were devoted to Wundt's Institute. Consequently I informed this kind friend that it was not my intention to become a professional naturalist, but that I wished to devote myself to the new science of psychology; that, however, I hoped to bring everything I might learn in his Institute and from his lectures into direct relation to psychology, and apply it to psychological problems. This aim was realized in my doctoral dissertation, which was judged by Leuckart as well as by Wundt.

Professor Leuckart remained kindly disposed toward me in spite of this explanation, and always followed my work with the same interest. The study of experimental psychology, however, did not then command the respect of all contemporary scientists, and often I felt as though Professor Leuckart, too, viewed the new science with a little disparagement. To this was added the fact that a former member of Wundt's Institute, who had distinguished himself notably in experimental psychology, became mentally deranged, an event which was interpreted unfavorably for the new methods of psychological observation. This case, however, had nothing whatever to do with such observations, as practiced in Wundt's Institute; a matter which presently was proved beyond doubt. I myself met this man at a later date, completely restored.

Rudolf Leuckart died soon after I had removed to Turin. To him I owe unwavering gratitude for all his goodness and kindness, for all I learned from him!

The enthusiasm which Leuckart's lectures as well as the practical work at his Institute awakened in me only strengthened my resolve to continue in my chosen path. Thus in the following semesters I attended courses on physics as well as on general and special anatomy and human physiology, likewise on psychiatry, on microscope theory, on motor phenomena in the vegetable kingdom, on the human function of walking, on the calculation of error and compensation, and so forth. Furthermore, I visited the Institutes of Physics and of Physiology, respectively, to gain practical insight there, and also frequented the Psychiatric Clinic, which was then under the direction of Professor Flechsig. He was then holding, besides his course on psychiatry, another on the structure of the brain, in which he was wont to demonstrate his discoveries in this field. My practical work in chemistry I carried on in a private laboratory. In short, I haunted all those institutes and lectures from which I hoped to draw the greatest benefits for the new science of psychology. To this day I am convinced that this sort of propaedeutic is the best preparation for any one who intends to devote his life to the study of modern psychology. My education, as I have sketched it here, was considerably furthered by the peculiar division of the German university calendar. Besides all these pursuits, I attended the philosophical lectures of Professor Wundt, and spent the afternoons working at his Institute. My interest was kept alive by innumerable new discoveries in all branches of natural science, as well as by the improvements that were constantly being made in regard to instruments and equipment; these gave rise to all sorts of discussion with the younger instructors and advanced fellow-students, discussions that were of value to all the participants. Without repeating familiar names, I need only mention the new developments which were then taking place in neurology, which could not fail to influence anyone concerned with psychological or psychophysical problems. In this way the semesters passed quickly, and every time we returned to our work with renewed enthusiasm.

My teachers in the medical and scientific branches knew what purpose my attendance at their lectures was supposed to serve. They all helped me and I owe them all a debt of gratitude. But I feel obliged to make special mention of the relation in which I stood, by reason of my visits to the Institute of Physiology, to Carl Ludwig, then its director, and to Max von Frey, his assistant at the time. To the latter I am bound by the ineradicable memory of many hours spent in collaboration. It was at the time when the well-known discoveries of Donaldson, Blix, and Goldscheider, concerning temperature areas of the epidermis, were receiving general notice. This discovery seemed to exalt to the dignity of an unquestionable law, at one stroke, the theory of the specific energy of the sense-organs, as outlined by Johannes Müller and developed by Helmholtz. The discoveries were met partly with doubt, partly with open opposition. As far as I am able to judge from my own experience, there can be no doubt in regard to the *general* facts as described by these scholars, though in regard to several details there is as yet many a question that requires solution. As regards the theory, I personally prefer to call it *the theory of the specific function of the sense-organs,* as the word "energy" seems to me to be irrelevant there. Energy and sensation are not the same thing.

Meantime the recently acquired knowledge of the analgesic effect which cocain has upon the mucous membranes was followed by the discovery of the anaesthetic effect of the acid contained in the leaves of *Gymnema silvestris* upon the taste areas for sweetness. Since I had been working for some time in Wundt's Institute on an extensive study of the gustatory sensations, this new discovery could not fail to excite my interest and give me a desire to test the findings for myself. The opportunity for such experiments was given me by Professor Ambron, who was then assistant at the Botanical Institute at the University of Leipzig, and who managed to procure for me a few leaves of the desired plant from Kew Gardens in London. I found the alleged effect duly corroborated by the experiment. This caused me to devote the summer vacation, which I spent alone at Wundt's Institute, to *a study of the effects of cocain and of gymnemic acid on the sensibility of the tongue and the mouth cavity;* a piece of research which was afterwards published by Wundt in the ninth volume of his *Philosophische Studien.* It was the first time that the sensibility of the mouth cavity and the tongue had been thus methodically investigated. In the course of these tests I discovered the area of the mucous membrane in the cheek which is insensible to pain, despite its unimpaired tactual sensibility. This painless zone has often been the subject of discussion because of its theoretical significance, although other experimenters and I have been able to demonstrate it again and again.

In this, my first piece of independent research, I was not really aware of the theoretical importance of my discovery. My knowledge of psychophysics was not equal to such an evaluation. I was eager only to establish facts which were not yet known. But the significance of the painless area of the cheek membrane was recognized by Max von Frey, who just at this time was busy with some researches whereby he hoped to prove that the sensations of cutaneous pain are not, as was generally supposed, produced through a heightening of touch stimuli, but required the assumption of some specific nervous equipment. Von Frey subjected the indicated area to a further test. Whereas I had discovered its anaesthesia through needle-pricks, he was able to corroborate it by the use of an electric current. This common interest led us to collaborate, which we continued later to do partly at Turin and partly at Zürich, whither von Frey was called. The chief result of our joint investigations lay in establishing the fact that excitement of the tactual organs of the skin is a function of the pressure obtaining at the spot in question (*Zeitschrift für Psychologie,* Volume 20, p. 126).

Amongst my most cherished possessions are also my memories of Carl Ludwig. His lectures had a wealth of demonstration that could not conceivably be surpassed. No sooner had a matter of fact been explained, but it was demonstrated. In the delivery of these lectures as well as in his preparation for them, which I was frequently allowed to witness, he exhibited a remarkable skill at vivisection, a skill perhaps equalled only by the greatest surgeons. At the same time he was always concerned to save the animal any unnecessary torture and to alleviate its sufferings by the use of cushions and other means of making its position more comfortable. I shall never forget, for instance, the grace and skill of his demonstration in the course of a lecture on the function of the heart when removed from the body. The attempt was as new as it was bold. It made a tremendous impression. I have frequently seen similar demonstrations in later years, but the impression of this first attempt by Ludwig has remained ineradicable in my memory.

Ludwig liked to attract zealous students by scientific conversations, and to awaken their enthusiasm for the pursuit of new scientific problems. Knowing that I was working in the field of sensation, he would often take me into his study after lectures, to discuss questions of natural science. Thus he tried to show me the necessity of subjecting the whole field of olfactory sensations to an entirely new investigation. He really won me over to the idea, and I began to make preparations for my prospective researches. But then Ludwig died, and almost immediately thereafter appeared Zwaardemaker's *Physiologie der Geruchs.* It was a great sorrow to me that Carl Ludwig did not live to appreciate this significant work, which really has put the physiology of smelling on its first scientific basis. One day, after a brief scientific chat, I accompanied him to the steps of his home, where he gave me a book to read, and then offered me his hand by way of farewell; I never saw him again. He contracted a sudden illness and died within a few days, deeply regretted by physiologists the world over, most of whom were probably his students or students of his disciples.

Ludwig's successor at Leipzig was Ewald Hering, who brought with him from Prague his assistant, the later well-known Franz Hofmann, now unfortunately no longer among the living. It was my good fortune to make Hering's personal acquaintance and to hear his lectures on his color theory, which always pleased me. I saw Hering for the last time in 1914, just before the World War, in the Leipzig Physiological Institute, where he showed me his new optical laboratory and himself demonstrated to me the uses of all his newest instruments. With Franz Hofmann, too, whom I liked exceedingly, I formed a close friendship and worked together with him for a while.

Meanwhile a change had taken place in Wundt's Institute. Dr. Kirschmann had been called to Toronto (whence the World War was to drive him back again to Leipzig), and Dr. Meumann had succeeded to his vacated position. Owing to some alterations in the old *Konviktgebäude,* the Institute itself was temporarily housed in a building belonging to the University, located on the *Grimmaischer Steinweg.* Professor Wundt himself, together with Dr. Külpe and me, directed the moving.

I had gradually completed my doctorate work in Wundt's Institute, and published my dissertation under the title, "Beiträge zur physiologischen Psychologie der Geschmackssinnes," in the tenth volume of the *Philosophische Studien.*

At the same time I became acquainted, through the aid of von Frey at Ludwig's Institute, with the method of graphic registration of pulse and respiratory motions, which, in view of the fact that they are co-variants with the emotional life, were attracting more and more attention among psychologists. Von Frey had written a book about the pulse. Other valuable works of the same sort had also appeared. The works of Angelo Mosso at Turin, which had appeared in German translation too (*Kreislauf des Blutes im menschlichen Gehirn, Diagnostik der Pulses, die Furcht, die Ermüdung,* *usw.*), created a stir. Alfred Lehmann's book, *Die Hauptgesetze des menschlichen Gefühlslebens,* had not passed unnoticed in psychological circles. At Wundt's Institute, Paul Mentz had completed a study of *Die Wirkung akustischer Sinnesreize auf Puls und Atmung* (*Philosophische Studien,* Volume 11). It was the time when the James-Lange theory was gaining ground, but encountering opposition from Wundt and his students. I had read Mosso's books, and had induced Wundt to purchase that scholar's much-discussed plethysmograph for the Institute. But it is not as easy as it may appear to work accurately with this instrument.

As Turin was generally regarded as the foremost place for the study of graphic methods, I conceived a wish to go there during the vacation of our German universities, after the winter semester of 1893-94, to acquaint myself with Mosso's instruments and the technique he had evolved. And since Professor Wundt likewise desired very much to introduce the new graphic methods at his Institute, he did not gainsay my plans. Moreover, I found a friend who, like me, wished to look into Mosso's labors and laboratories. With this friend, who unfortunately is no longer among the living, I travelled to Turin, at the end of February, 1894.

I went first to Strasbourg, where I made a brief visit at the Physiological Institute of Professor Goltz to see some dogs upon which he had operated, and which were then arousing much comment; thence to Basle, the home of my friend, who was awaiting me there. After only a few days' stay in the city of Böcklin, on the shores of the Rhine, we continued our journey to Turin, where we arrived late in the evening. The very next day saw us at Mosso's Institute, and, almost immediately, hard at work.

Mosso had only recently occupied his new Institute, which at that time was generally considered one of the finest. He had all the newest conveniences and arrangements. A man of about forty, he was at the height of his working capacity. His assistants were Valentino Grandis, Zaccaria Treves (now both dead), and Mariano Patrizi, at present Professor of Physiology at Bologna. His technician, who had his shop in the Institute, was the well-known Luigi Corino, who died only a few years ago—a very competent and likeable man, who took part in all Mosso's researches and was always available for the construction of experimental outfittings. He made all Mosso's instruments. He helped me too, in later years, in the furnishing of my own psychological institute.

Mosso, as well as all the officers of his Institute, gave us the kindest sort of reception. As for the man himself, we could only marvel at his enthusiasm for research work, and at his energy which never seemed to flag. He, too, was one of Ludwig's students, and had worked for two years in Ludwig's Institute, during the decade 1870-1880. To this time, also, belong his earliest plethysmographic experiments.

At Mosso's Institute I acquired the technique of the plethysmograph, as well as his other instruments (such as, e.g., his ergograph). But as his technician had just furnished him with a new instrument, the so-called sphygmomanometer, he advised me not to work with the plethysmograph, but to use this new invention for an experimental study of the changes of blood-pressure with mental excitement. Time was lacking, however, to complete this work at Turin. Mosso gave me the instrument to take home as a present to the Leipzig Institute. It was there that I brought my experiment to something like a close. The result appeared in the *Philosophische Studien* (Volume 11) as well as in Mosso's *Archives.*

I should like to add that Mosso's sphygmomanometer became the basis for the construction of Lehmann's plethysmograph (better called hydrosphygmograph). This instrument is indeed based upon the very same principles as Mosso's sphygmomanometer. Lehmann, a former pupil of Wundt's, first saw the apparatus at the Leipzig Institute, while I was working with it. It was chiefly on the strength of Lehmann's results with his new instrument that Wundt later formulated his new theory of the emotions.

After my return to Turin, my fiancée, Miss Emma Lough, and I translated Mosso's book *La Paura* into English. It is needless to say that the greater part of this work was hers, and that I merely revised the passages as she translated them. This English translation (*Fear*) was published by Longmans, Green, and Co. in London, in 1896. At the same time I continued to work in Wundt's Institute on sensations of taste and on temperature spots in the skin. Likewise I assisted Professor von Frey at the Physiological Institute with his rearches on epidermal sensations. My work on taste and temperature sensations appeared in Wundt's *Philosophische Studien.* Von Frey's work was published by the Saxon Royal Scientific Society of Leipzig (*Königl. sächsische Gesellschaft der Wissenschaften zu Leipzig,* Volume 23, Section on Mathematics and Physics, 1896) under the title, "Untersuchungen über die Sinnesfunktionen der menschlichen Haut." This work has become the foundation for other inquiries along similar lines.

Further changes occurred among the members of the Psychological Institute during the year 1894. Oswald Külpe was called to Würzburg as full professor at the University there, and at the Institute Dr. Meumann took his place as chief assistant. The second assistant had, so far, been privately retained by Wundt. But as the Institute, by its change of quarters, had gained considerably in point of space, and as the number of students was steadily increasing, the ministry at Dresden conceded it another university assistant, and this position was given to me. In this capacity I continued to work at Leipzig until the spring of 1896. Then Angelo Mosso offered me a position at the Physiological Institute at Turin, as Assistant in Psychology, with prospects of further advancements in Italy. This offer appealed to me chiefly because it would give me an opportunity to devote myself independently to the elaboration of the science of psychology. In March of that same year I was married, and, this time in the company of my young wife, I took my second trip across the Alps, to Italy, where I was to find my second home and my permanent, definite sphere of activity.

I have let my story run somewhat ahead of an event which was of highest importance to the development of my psychological views; let me therefore return once more to Leipzig, and review the antagonistic attitude which Külpe gradually assumed with regard to Wundt's fundamental conceptions. This defection was not apparent until the publication of Külpe's book, *Grundriss der Psychologie,* in the year 1893. It was Külpe's first book, and furthermore the first presentation of psychology on an experimental basis, after Wundt. The first inspiration of this work had come from Wundt himself, who needed a textbook for his numerous and steadily increasing classes. At the same time, he hoped to give Külpe an opportunity to be promoted to the rank of Assistant Professor (*Prof. extraord.*), which promotion, according to the regulations of the Department of Philosophy at Leipzig in that day, demanded the publication of some work of book-length. And indeed, Külpe received his promotion, on Wundt's recommendation, immediately after the appearance of the book.

Külpe had often talked to me about his work in the course of writing, and had more or less prepared me for the fact that in certain respects it would not quite agree with Wundt. Consequently I looked forward to its publication with some eagerness. At last it came off the press, 478 pages strong, published by Wilhelm Engelmann at Leipzig. The author denoted his psychology as "Psychology of the Human Individual," or as "General Psychology." Animal and social psychology were excluded from his treatise, being mentioned merely in a few words. Likewise the psychology of the human child. A somewhat longer section was devoted to "The Pathology of Mental Life" (p. 16) and to "Mental Development" (p. 18).

Külpe had dedicated the work to Wundt, who received it with obvious delight, and with the words: "It is a fine volume!" But when he actually began to read it, he was doomed to a disappointment which could not possibly have been worse.

Anyone familiar with Wundt's fundamental point of view must realize that his whole psychology is dominated by the idea of *psychic causality.* Without this assumption, all psychical phenomena are, according to Wundt, incomprehensible. For him, psychic causation is the fundamental law of all mental process. There was scarcely a product of his tireless researches to which he attached so much importance as to this law, of which he had had the first inkling even in his early youth, in the creative synthesis which was so vital to his whole system. To no part of our science had he devoted more energy than to just this fundamental law. Just as it guaranteed him the independence of his science from physiology, so it also led him, on the other hand, to grant to psychology a position among the mental sciences comparable to that of physics among the natural sciences. With this end in view we can understand his intense struggle for the acceptance of psychic causation, and of the principles based on this concept: *the actuality of all psychical processes, the creative synthesis and growth of spiritual values, relative analysis* (beziehende Analyse), *enhancement by contrast* (Kontrastverstärkung), *the heterogeneity of purposes* (Heterogenie der Zwecke).

If one bears these facts in mind, it is not hard to understand the disappointment Wundt must have experienced in perusing Külpe's book. Comparing it with the views expressed by Külpe, e.g., in his treatise on the will (*Philosophische Studien,* Volume 5, pp. 179, 381) there had really been a change of front. Yes, one might say that his *Grundriss* actually culminated in the proposition: "There is no psychical causality." Even the demand he makes upon psychology in the introduction to his book allows of no doubt upon this matter, for there he says (p. 6), "If we mean, by a theory in the spirit of natural science, a statement of the conditions under which a phenomenon takes place, then the theory of psychical processes is obliged to furnish us with a proof of their dependence upon certain bodily processes." Although Külpe did not overlook the difficulties attendant upon such a demand, and was quite conscious of the fact that a rational theory of psychology according to his conception was not yet feasible in his day, still he clung to his ideal and tried to suggest ways and means by which such a theory could be at least foreshadowed.

Likewise in his critical treatment of the association theory (p. 198), after repudiating Herbart's ideation-mechanics (*Vorstellungsmechanik*) and then calling attention to the new results of brain physiology and pathology, he writes: "If, however, there is really such a relation of dependence of associated ideas upon brain-processes, then any special causal connection of the ideas with each other may be dispensed with. Everything in favor of such a theory may then be traced far more simply and economically to the fact that certain localized physiological processes are causally interrelated. Above all, it is through this assumption only that we can explain those aberrations which, by the evidences of experience, without any metaphysical aid, call the causal nature of these associations seriously in question."

Utterances of this sort were generally understood to imply that the task of scientific psychology was not first and foremost to account for the facts of consciousness and their significance for the development of individual and collective mental life, but that its aim was to determine the physiological excitations which underlie the psychological facts, and then merely to describe the latter. Within such a science there was, of course, no room for any proof of a psychic causal order.

In this spirit it was that Götz Martius, among others, interpreted Külpe's exposition, and opposed him with harsh criticism (*Zeitschrift für Psychologie,* Volume 9, pp. 23-45). "The exaggeration of the material aspect," Götz Martius writes in concluding his attack, "leads inevitably to the void of materialism or the abyss of the Unconscious." . . . . "Let psychology content itself with the observation of the facts of consciousness, and accept the inner life as its authority under all conditions."

To go into more detail would take me too far afield. Let me merely remark that Külpe offered a further critique of Wundt's theory of compound reactions (*zusammengesetzte Reaktionen*), which was refuted afterwards by Emil Kraepelin and Julius Merkel (*Philosophische Studien,* Volume 10, p. 499).

It was a lively time at the Institute! In a way our sympathies were largely with Külpe, who was very popular, owing to his genial and sociable disposition; on the other hand, there was our profound respect for Wundt, for his greatness and the wealth of his learning, which he was always ready to put at anyone's disposal. Thus it was quite the usual thing, after this, for little groups to form here and there in the Institute, hotly discussing the problems which had been agitated by Külpe. Outside the Institute, opinion was divided. And it was just as always in such a case—to some his innovations seemed too radical, and to others, not radical enough. From my various talks with Külpe himself I gathered that he felt generally misunderstood.

The question was often raised, by what influences Külpe had been brought to his new point of view. In this respect people were almost without exception upon a wrong track: he had, indeed, written his book under the influence of Mach's *Analyse der Empfindungen* and Avenarius' *Kritik der reinen Erfahrung.* He called attention to this fact in his second work, *Einleitung in die Philosophie,* which appeared as early as 1895, and where he offered his fundamental point of view in briefer and clearer fashion than in the *Grundriss.* At the same time one could detect in the second work a certain softening of some of his former statements, at least a formal modification.

"Just as a continuous curve in analytic geometry may be given as a function of two variables, the abscissa and the ordinate," he writes in his second book (p. 63), "without impairing our appreciation of the curve as a continuous entity, so the concrete unity of our world of experience is in no wise destroyed by a similar analysis of every event into a subjective and an objective factor." To empirical psychology he relegates also the task of tracing complex psychic phenomena back to simple ones. The difference between nerve-stimulation and sensation is duly recognized by Külpe, so that one cannot fairly accuse him of any materialistic tendency. The question of *psychic causation* he proposes to leave to a *philosophical* psychology. (p. 69). What prevented him from granting such a principle was the fact that, ultimately, he assumed a mutual relation of dependence between the physical and the psychical, which in the further course of his development let him even assume the possibilty of some sort of *psychophysical* causation. This he conceives as a *psychophysical* causal relationship without any exchange of energy, but cannot in any way describe this process in further detail (cf. the 8th ed. by A. Messer, p. 252).

It is not surprising, in the light of this dualistic interpretation of the mind-body relation, that Külpe repudiated the activity theory and inclined to the substance theory (cf. 8th ed., pp. 349 f.). We cannot enter here into any detailed proofs of his theory, especially as such proofs are, after all, mainly dialectic, designed to convince the theoretical intellect—as surely they must have convinced Külpe—but not to furnish undeniable facts. So I will merely emphasize the fact that I, standing as I did between Külpe and Wundt, finally struggled through to a fundamental acceptance of Wundt. Our science, as I see it, includes two great spheres of research: on the one hand we must determine the conditions necessary to the appearance of psychological phenomena as such; on the other, however, I believe we cannot evade the activity theory. And this theory, in spite of our recognition of psychophysical parallelism as an empirical-heuristic principle, leads us—by application of the law of sufficient reason—necessarily to the assumption and acceptance of a *psychic* causal order. Of course we all feel a desire to unite these two complementary causal orders. But such a conciliation for the satisfaction of feeling and intellect can scarcely be considered the task of scientific psychology, but belongs to scientific metaphysics, which commands both fields at once and draws conclusions from the *general* results of their respective researches.

In spite of his departures from Wundt and the polemic which he directed against his master, one can always recognize in Külpe's work a disciple endeavoring to lead us to a standing-ground beyond that of his master. This is evident, for instance, from his much-discussed experiments with thought processes, to which he gave his attention during the second period of his career. Concerning our religious needs, I do not believe that they can be satisfied through either the soul theory of Descartes, the monads of Leibnitz, or the "Real" of Herbart, or, for that matter, any philosophically formulated concept. Külpe, too, must have had some such impression, when he raised the question, apropos of the psychological problems arising in philosophy, whether one could not oppose a *dynamic* theory to the substance-conception and regard the soul as a peculiar center of force. But even this would not, in my opinion, offer us much of a solution.

The gulf between our very real religious yearning and our scientific needs can neither be bridged by empirical psychology, nor by metaphysics, which do not dispose of incontrovertible facts. Even Külpe had finally to admit that any attempt to give a satisfactory reasoned account of the nature of the soul was premature.

Külpe's *Grundriss der Psychologie* inspired Wundt to undertake a restatement of his doctrine of *psychic causation.* This extensive treatise appeared the very next year (1894) in *Philosophische Studien* (Volume 10, p. 1), and may also be found in his *Kleinere Schriften.* As, furthermore, Külpe's book had not satisfied his need for a textbook, he decided at about the same time to write an introduction of his own which should supplement his lectures. That was the origin of Wundt's *Grundriss der Psychologie* (1896). It is noteworthy that it bore the same title as Külpe's book of the year 1893. Wundt composed this outline with particular care. It presents his doctrine in architectonic completeness and brevity. The last section of it is devoted to his theory of psychic causation, which finds its further development and justification in the ten volumes of his *Völkerpsychologie* as well as in the later editions of his *Physiologische Psychologie,* his *Logik,* and his *Ethik.* The little work met with an excellent reception. Several printings had to be issued in rapid succession. The fourteenth edition appeared in the year of Wundt's death, the fifteenth was edited by his son without any deviation from the previous one, which had been the last to be issued by the author himself. The posthumous fifteenth edition contained a bibliographical supplement by Professor W. Wirth of Leipzig. The book has been translated into several languages: the first edition into English, under the supervision of Wundt himself, by Charles H. Judd, the third into Italian, under my direction, by L. Agliardi.

This great success which greeted Wundt's little work probably furnished the main reason why Külpe never could make up his mind to let his own *Grundriss* appear in a second edition. He could not and would not enter into competition with Wundt. But considering the variety of stages through which Külpe's mind passed in the course of its development, I cannot but feel that, had it not been for his untimely death, he would have drawn closer again to Wundt in many respects.

In Turin I met with the greatest kindness from everyone. Mosso assigned me two rooms at the Physiological Institute, where I could begin my activities. These two rooms were the commencement of the Psychological Institute of Turin. Here I translated Mosso's latest book, *Fisiologia dell' uomo sulle Alpi,* from the Italian, and William James's *Talks to Teachers on Psychology* from English into German. At the same time I began to gather a small group of young people about me, who aided me in my experimental work, and whom I advised as to the proper approach to psychological problems. To this circle belonged Luigi Agliardi, Mario Ponzo, Arturo Fontana, Raoul Hahn, Luigi Botti, and many others. The first in this list, as I have already mentioned, was the Italian translator of Wundt's *Grundriss*. He aided me in the preparation for my later experiments on reaction-time. Mario Ponzo was interested from the start in graphic methods, which he afterwards brought to a high degree of perfection in the course of his researches on the expression of volitional impulses in respiration-curves, as well as through other works performed at my Institute. Besides this, Ponzo afterwards carried out many experiments concerning skin sensations, position sensations (*Lageempfindungen*), and others, under my guidance and assistance. Since the foundation of my own Institute, he has been my collaborator; he is now Instructor in Experimental Psychology. Luigi Botti, who is now Instructor in Theoretical Philosophy, devoted himself especially to a broad investigation of optical illusions, which also was not concluded until later years, at my Institute. Arturo Fontana (now likewise an instructor at the University) made extensive studies, under my supervision, on sensations of the skin, and later became a dermatologist. With Raoul Hahn I made many experiments on the sensibility of the mouth cavity. He became an instructor in ear, nose, and mouth diseases, but died a few years ago.

My activity at Turin was interrupted at about this time by a short professional trip to Zürich, whither Professor von Frey had been called. Here, together with him, I completed the work on *the function of touch corpuscles,* which I have already mentioned. Besides several other works that I carried out in Zürich, I had an opportunity at last to realize a wish which I had long entertained, but which required the cooperation of a chemist; and that was to answer the question to what extent gustatory qualities are determined by the ions of the stimulant substances. This cooperation was given me by Rudolf Höber, then the Assistant at the Physiological Institute of Zürich. Our article appeared in Ostwald's journal, the *Zeitschrift für physikalische Chemie* (Volume 27,

p. 601). This brings to my mind the pleasant memory of Professor Martius' lectures on anthropology, which I then had opportunity to hear.

Ten years I worked in Mosso's Institute, as head of the little Department of Experimental Psychology. Here I completed my treatise on freely arising images, etc. (*Archiv für die gesamte Psychologie,* p. 357), the conclusions of which I am still ready to defend. In the year 1899, the Faculty of the University of Turin admitted me as an Instructor (*libero docente*) in *General Physiology.* Two years later I was appointed by the same faculty to teach *Experimental Psychology.* At last, in the year 1906, our Minister of Education, the late well-known psychiatrist, Professor Leonardo Bianchi of Naples, managed to introduce simultaneously in Rome, Naples, and Turin, independent chairs of experimental psychology. I competed for the professorship in Turin, and obtained the chair. The new professorship was assigned to the Department of Philosophy, and has remained ever since in this connection. The difficulties of establishing the new institute were considerably diminished through the fact that one of my most talented students, Emilio Pellegrini, of whom unfortunately I was deprived by death, had undertaken, before his untimely end, to assist me financially, and charged his family with the execution of his wishes. In his honor the institute bears the name, *Enrico Emilio Pellegrini Foundation.* Quiet rooms, such as one needs for the performance of psychological experiments, were at our disposal in a former monastery, which had harbored Mosso's Institute as well as other University laboratories, until these were all removed to the new buildings at Valentine Park. At my Institute, quietness is guaranteed to me by the fact that all our laboratories lie toward the cloistered courtyard. Besides the laboratories and the library, my Institute possesses a special lecture hall.

Of the younger scholars who worked in my Institute, I might mention, besides Ponzo and Botti of whom I have already spoken, Dr. Leopold Chinaglia, who fell in the World War. Among other works which he carried on at my Institute, he did some experiments, under my direction, on the influence of temperature on sensations of touch produced by objects laid upon the skin. As he came to us from the Department of Natural Science, and had already done some research in zoölogy, I tried to interest him in animal psychology. He was enthusiastic about the subject, and collected a great deal of material concerning insects which had been observed under various conditions; his family is still in possession of these notes, but as he himself did not live to arrange them in their proper order, they are lost to our science. Had this brilliant and zealous young scientist lived, Italy would have had in him an expert on animal psychology of no mean order.

Of the remaining persons who worked in my Institute, I must further mention the present rector of the Catholic University at Milan, the Franciscan monk, Professor Agostino Gemelli. As it was his wish to continue his education along psychological lines in Germany, I recommended him, at his own request, to my friend Oswald Külpe, at whose institutes in Bonn and Munich he worked, acquainting himself especially with Külpe's methods of studying thought processes. He was promoted here at Turin to the rank of Instructor in Experimental Psychology, and later founded a very well-equipped Institute for Experimental Psychology and Biology at the Catholic University of Milan.

For the sake of brevity, I pass over a number of other young workers who frequented my Institute, and will mention only the achievements of one of the youngest of my students, who also came to us from the Department of Natural Science, Dr. Alessandro Gatti, who is just now in the United States as Fellow of the Rockefeller Foundation. In order to decide the much-debated question whether Weber's law may claim general validity, I suggested to him that he might experiment on *tactual* difference sensations in the simplest possible conditions, i.e., to investigate whether the law holds good in the working of a single organ of touch. This investigation required unusual concentration of attention on the part of the observer. By its results, the question was affirmatively decided. Gatti, too, attained the rank of Instructor in Psychology (1926). On my seventieth birthday he presented me with a new treatment of the question, which he had worked out at New Haven with the aid of the touch-balance constructed by Professor Dodge. This work, which includes a description of the instrument by Professor Dodge, has just appeared in the *Archiv für die gesamte Psychologie.* The experiments have led once more to a perfect corroboration of Weber's law. We are not surprised if a somewhat recondite phenomenon in physics or chemistry, histology or physiology, or any other science of the external world does not appear in pure form unless every possible source of error be obviated; the less strange should it seem to us that the constancy of difference sensibility cannot be accurately observed unless all possible errors, internal or external, be excluded. This constancy is not attained unless the observer is very skilled in the evaluation of very small differences of intensity. Weber's law is for me a fact which in the last analysis cannot be explained on any but a psychological basis. In a further study, concerning the subjective evaluation of the central point in plane geometric figures, Gatti demonstrated that the characteristic error committed in such observations rests on a perfectly analogous relation. Furthermore, he has been occupied also with eidetic problems and with social psychology (*Völkerpsychologie*).

Speaking of social psychology, which also owes its present form to Wundt, I may mention that I have frequently conducted a course on this subject, apart from my lectures on General Psychology. Upon my recommendation and through my active efforts, Italy is now in possession of a translation of Wundt's *Elemente der Völkerpsychologie* (*Anchieri, Fratelli Bocca,* Turin). The interest which this branch of our science has aroused in some of our younger scholars allows me to hope that Italy, which produced the real founder of social psychology, G. G. Vico, will not lag behind in the solution of problems in this special field.

Applied psychology is represented at my Institute chiefly by Dr. Ponzo, who has given much attention to questions of psychotechnics and has sometimes given a special course on this subject. Also, he has recently published an *Introduction to Psychotechnics.*

Without wishing to go into any detailed discussion of my own works, I ought to mention, however, that lately I have devoted myself chiefly to the study of eidetics and the problem of Gestalt. The theory of eidetics as developed by the Marburg school contains certain truths but also great exaggerations. Eidetic phenomena are to be found not only in the realm of vision, but also in other departments of sense, and doubtless are related to very special personal dispositions. After testing more than a thousand children of different ages recently, I have come to the conclusion that the distinctions which obtain between so-called visual memory and optic eidetic images are not completely definite in childhood. Regarding the method of *graphic reproduction* which I have originated, and which can well be used for the discovery of optic-eidetic inclinations, let me say that my latest experiments have shown that *colored* tests are better for this purpose than *non-colored* ones. The latter may also be reproduced by a good ordinary memory type, whereas colored tests require an eidetic tendency for their reproduction. As for the *latent* optic-eidetic tendencies which the Marburg school claim (quite rightly, I believe) to have observed, I should say according to my own experiments that these cannot be explained as a deviation from *Emmert's law,* but follow the well-known *Mendelian* principle. The terminology used by the Marburg school to denote the distinct types which have been recorded "tetanoid" and "basedowoid" types) does not seem to me particularly happy. The differences in type are, as far as I can see, relative to a more or less strongly developed imagination. Since we are here dealing not only with pathological cases but also with perfectly normal phenomena, as the Marburg psychologists themselves have demonstrated, it is not advisable to use expressions derived from abnormal psychology, which may too easily lead to an erroneous conception. As for the somatic peculiarities which the Marburg school requires for the recognition of the particular types, I have discovered no relationships of this sort. My contributions to the problem of eidetics, so far, have been published in the *Archiv für die gesamte Psychologie* as well as in the *Archivio italiano di Psicologia.*

Concerning the problem of Gestalt, which has created such a stir as almost to assume the importance of a crisis in the field of psychological research, I must confess that the fundamental ideas upon which this whole problem is based do not inspire me with confidence, either from the psychological or the epistemological angle. Without wishing in any way to disparage the actual, *positive* results of Gestalt psychology, I still maintain that it is impossible, without the assumption of *elementary* sensations and of *elementary feelings,* to explain the experiences which are always given to us in more or less complex form. From this it follows that Wundt's theory of a creative psychological synthesis cannot be refuted by the theory of Gestalt. In my opinion, Gestalt psychology has so far been altogether too closely bound to the observation of visual phenomena, and has neglected the other departments of sense, as well as their interaction. Just now it looks as though the word "Gestalt" were serving the purpose of a slogan, by means of which the most arduous problems are all at once to be solved. Slogans have never yet advanced science. To this we may add the fact that the various representatives of the new theory tend to differ among themselves. Epistemologically, the proceeding of the several adherents of the theory seems to me to indicate a return to a naïve realism in the interpretation of perception experiments, which the joint efforts of the best epistemologists of our day seemed long since to have transcended.

The problem of Gestalt, along with the problems of emotion and of the will, is perhaps the most absorbing subject in present-day psychology. Its importance, of course, cannot be denied. It is one of the fundamental problems of our science. We are dealing here, when all is said, with the problem of perception, which as yet contains many partial problems and can be solved only by careful research in *all* sensory fields. This solution will not be accomplished without heated controversy, but then, no great truth has ever been attained without a contest.

I have been asked which problem I would tackle with the greatest hope and enthusiasm if I were to be given a second youth and another opportunity to enter upon the field of psychology. It is hard to reply to such a question. But as an answer is apparently expected from me, I may safely reply that, without neglecting other problems, I would devote myself specially to the problem of feeling, and the theory of sensation and perception as a whole; that is to say, that besides the study of psychological phenomena as such I would pursue as far as possible the physical and anatomico-physiological conditions on which the very presence of any psychological phenomenon depends.

I have reached the end. The new science of psychology is not yet a century old. From modest beginnings it has risen, despite many hostile attacks, to be a great power, which includes certain departments of medicine, and can certainly no longer be despised by any other empirical or rational science. For all scientific achievement is a function of the human mind, with its various delusions and mistakes, and it is the task of our science to determine the powers and limitations of the exploring human mind. When the century is completed, others will fill our places. These I welcome from afar, uplifted by the conviction that they, too, will be imbued with the consciousness of the high mission entrusted to the science of psychology.

WILLIAM McDOUGALL

My great-grandfather in the paternal line began life, I am told, as a cobbler in his native wilds. He seems to have been a man of some spirit and originality, for he eloped across the border with an heiress and settled down in the north of England. One of his sons, my grandfather, early became the proprietor and headmaster of one of the old-fashioned boarding schools for boys. He was interested in chemistry, and this interest led him to become a pupil of John Dalton, the author of the atomic theory, and an intimate friend of Angus Smith, famous in applied chemistry, and of Sir James Simpson, the Edinburgh surgeon who first applied chloroform as an anaesthetic. He was interested also in agriculture, and he set out to apply his chemistry to the improvement of that art. He bought a tract of land beside his school and there developed a chemical factory in which were made a number of the products he had himself devised. He took his five sons into the business which soon did and still does a world-wide trade.

I remember my grandfather as a stern and very pious old gentleman whose hobby was the writing of articles to show that the Bible miracles were compatible with the teachings of science. I remember that, even as a young boy, I regarded this as a somewhat futile labor.

My father was a typical dark Highlander, that is to say, of the Mediterranean type, small, dark, long-headed, fiery, and markedly extroverted. His features were regular and well cut, and he was not without a harmless vanity. He had an active mind with a streak of originality. Although he became chiefly responsible for the chemical business, which brought in large profits, he built up alongside it an iron foundry in which to manufacture his own mechanical inventions, and later a paper-pulp factory for the same purpose. And in these enterprises he spent a large part of his profits. The same traits were shown in his religious life. He was successively a member of most of the leading Christian sects; and in his later life adhered to none, preserving a friendly and respectful neutrality towards them all. He was benevolent and affectionate, with a strong taste for poetry and music; as in his business, so also in religion, art, and domestic affairs, he was masterful, erratic, unpredictable, and always naïve. Though sometimes hasty, his anger did not endure, and he was always an indulgent father to his daughter and four sons. From an early age my respect and affection for him were tinged with a critical amusement. He was fond of denouncing the clannishness of the Scottish highlanders, yet showed the trait strongly in freely aiding large numbers of poor relatives.

My mother was of pure Saxon type, as were both her parents. They came of a long line of yeomen who probably had cultivated the same field since the days of the Saxon Heptarchy, without marrying outside of their own group. Both she and her mother were strikingly beautiful examples of the fair, calm, introverted Nordic. Some of her brothers were distinguished athletes. She combined all the virtues and was in every way an ideal wife and mother. Her defects were purely the defects of her qualities; she lacked the touch of erratic originality so strong in her husband.

I thus represent that blend of the Mediterranean and Nordic races which has produced the English people. But, whereas most Englishmen come from a crossing that took place many generations ago, I am of the first generation of crossbreds, what the geneticists call the F_1, generation. I am inclined to attribute to this the fact that I have never felt myself to be altogether and typically English or altogether at home in the English social atmosphere. I have, I believe, inherited about equally from both parents, and my constitution seems to comprise elements from both sides which have not been sorted out, as in the products of older crosses, into an harmonious pattern. To this I attribute the fact that I have never fitted neatly into any social group, never been able to find myself wholly at one with any party or any system; and, though not insensible to the attractions of group-life, group-feeling and thinking, have always stood outside, critical and ill-content. I have participated in the life of many groups, scientific, medical, academic, and social, but have belonged to none. Consequently, the list of my acquaintances on both sides of the Atlantic is immense; but I have very few intimates, and have always stood alone in my intellectual interests. This isolation has been an involuntary outcome of my nature, which I have learned to accept as inevitable.

I was a precocious child; and it was early made plain to me that I was expected to distinguish myself. At five years of age I went to a private boys' school where for some years I was the youngest pupil. I reacted by becoming somewhat domineering to boys of my own age; and I well remember my astonishment when, on taking by the ear a boy considerably older and bigger than myself, the worm turned and gave me a drubbing.

The headmaster of the school, an Episcopalian clergyman, was an admirable character and teacher. I still regard him with great respect and affection. I excelled in "Euclid" and could easily master the language lessons, beginning Latin and French at six years. Science was introduced as a weekly treat in the form of Huxley's little book on physiography, and I enjoyed it greatly. Before leaving the school at fourteen years I had begun to read extensively the better English novelists and to make acquaintance with some scientific classics. Hume and Gibbon were on my father's shelves; and, though they were bugbears to the prevailing non-conformist conscience, he encouraged me to read them. My mother saw to it that the house was supplied with the best magazines and current literature.

At this time the family had moved from the neighborhood of the chemical works in the devastated Lancashire country-side in order to seek the educational advantages of a city. We lived in a large house in an outer suburb of Manchester. We had a garden with tennis court—then a novelty—and a large paddock and stabling. My father was fond of horses and kept several. He lived in the style of £2000 a year, which meant at that time a very comfortable ménage with four or more servants.

In those days the northern manufacturers were a class apart from the rest of the English social system. They were class-conscious, conscious of power and of their peculiar interests. Bright, Cobden, and Gladstone were their leaders. They regarded the public schools and the universities with a doubtful eye as strongholds of Toryism. My father shared these views; hence, when at fourteen I had absorbed what my school could give me, my further education became a problem. Germany had a high reputation for learning and education. So I was sent with my elder brother to spend a year at Weimar. There we attended the *Real-Gymnasium* and acquired the German language, an acquisition that has always been useful. In other respects, the year in Weimar was, I think, disturbing to my intellectual development. I was too young to appreciate the history and social system of the country. No doubt, we profited from regular attendance at the excellent theater and opera; but the language difficulty and the great differences in methods of instruction prevented my making as much academic progress as I should have made in a good English school.

On my return home in the summer of 1886, the problem of the next step arose. My father, proud of my precocity and noting that

I had a biting tongue, projected for me a brilliant career at the bar, culminating in the Lord Chancellorship. For in England the bar offers to the young man of brains, who can afford to work without earning until he is thirty-five or forty, the most brilliant rewards, wealth, fame, and titles. But I already was ambitious to do something worth while, and I was so arrogant as to think that the work of the successful barrister had no particular value. My father's alternative plan for me was that I should become an expert chemist and aid him in the development of his chemical industry. This plan, also, though it promised to bring at least wealth, I was too proud to accept; and my mother supported me in my objection. A career in pure science appealed both to her and to me as the most desirable. So I entered the recently constituted University of Manchester at the absurdly early age of fifteen years, and attended there during four years, continuing to live at home. The Faculty was strong on the scientific side; all the professors on that side were then, or later became, fellows of the Royal Society. I soon acquired the ambition to see myself a fellow of that august society and to write after my name the magic letters F. R. S.

At first I attended classes, mainly in the languages, history, and mathematics, but soon was drawn to biology. I read widely and before my first graduation in general science at seventeen years of age I had read nearly all of Spencer, Darwin, and Huxley, Lyell's *Principles of Geology,* and other standard works of science. The great controversy between evolutionary theory and religion was still raging, and I delighted in Huxley's smashing attacks on Gladstone and all the orthodoxies. In spite of my father's versatility in religious matters, he still led his flock regularly to church. At sixteen years I said to myself that the teaching of the Christian Churches was either a matter to be taken very seriously or a monstrous system of delusions. For a brief period I inclined to take it very seriously. But my reading turned me the other way, and I soon found myself very skeptical. I never, like Shelley, declared or felt myself to be an atheist. I had never been persecuted; I had no resentment against the Church, and no father-complex to prompt me to rebellion. My indulgent, erratic, rather brilliant father had never ruled me. I was a little exasperated sometimes by his inconsistencies; but, while appreciative of his qualities, I viewed his weaknesses and eccentricities with kindly tolerance. It is, I think, this relation to my father which makes for me now the whole elaborate Freudian structure of the father-complex seem purely mythological and unreal when propounded, as it is, as a universal factor in the life of mankind.

The attitude to religion thus early acquired has never varied greatly. In those days the word "agnostic," recently popularized by Huxley, seemed to me the best banner under which to sail. But my agnosticism was not militant, aggressively negative, or hostile to religion. I said *ignoramus,* I could not follow Dubois Reymond in adding *ignorabimus.* It seemed to me that most of the men who took life seriously and worked for the improvement of the life of mankind were in one sense or another Christians. And so, though the moral and historical bases of Christianity seemed to me incapable of resisting any serious examination, I did not feel that the intellectual was either justified in attacking religion or required to make a public display of his own skepticism. I saw that, though it was impossible to prove the truth of any of the propositions taught or implied by the Churches, it was equally impossible to prove that there was no truth in them. In my fourth year at the Manchester University I specialized in geology, led thereto by the fine museum and the fascination of palaeontology as one of the great approaches to the study of evolution.

During these years, though I was a very serious youth, I did not scorn athletic pursuits. I represented my University in Rugby football, on the track, and on the river; and in the vacations I rode, swam, played tennis, and climbed mountains.

One effect of these years as a daily attendant at a university in a great, ugly, smoky manufacturing city was to engender in me a violent dislike of all such cities and a passionate love of natural beauty, especially of mountains and the sea. Our family was tied by my father's business to the neighborhood of Manchester where his main offices were. But he provided compensation in many holidays spent in the western Highlands, in the Lake country, in the Welsh mountains, and in the Alps. I became a disciple of Wordsworth before I had read his poetry.

In other ways I was ill-content with my provincial university. What I read of Oxford and Cambridge fired me with a strong desire to study in one or other of those antique seats of learning. Their academic and social prestige were immense; and they were turning to the study of Science. Cambridge seemed to me the home of first-rate minds, Oxford of the second-rate. Chaucer, Milton, Cromwell, Pitt, Newton, Gray, Wordsworth, Tennyson, Darwin, all these great men had dwelt and worked at Cambridge; and I must do the same. My father's prejudice against the Tory strongholds continued; but he consented that I should go up to Cambridge if I could show myself able to win a scholarship there. So in December of 1889 I presented myself for the scholarship examination at St. John's College, Cambridge, and was offered a scholarship which I eagerly accepted. This, my first, visit to Cambridge enchanted me. The beautiful old courts, rich with memories of men who had played great rôles in the life of England, the rural surroundings, the primitive simplicity of the little town, all contrasting so violently with the environment of provincial commerce and industry which I had found so repugnant, combined to charm me. And so in the fall of 1890 I went up to Cambridge to make a new start, and, as it seemed to me, on a higher plane.

My position was a little unusual. I was a freshman just turned nineteen. My fellow freshmen were for the most part fresh from school, while I had graduated with first-class honors from a provincial university. While they had the childish outlook of the average public-school boy, I was in many ways extraordinarily mature. The result was that I lived a double life. As a freshman I took part in and enjoyed the many boyish activities that make the daily round of the average undergraduate; I joined all the clubs and rowed in the college boat; I wined and sang and played cards. At the same time I looked on critically, despising a little these pursuits as somewhat childish. The Dons, seeing my participation in the social and athletic life of the college, wrote me down a lost soul. But I achieved a certain prestige among them by passing the Little-go with a first-class in Latin and Greek at the end of my first term, although I had studied no Greek before joining the college. And a few of them soon discovered the relative maturity of my mind and interests and gave me their companionship. During this freshman year, in my desire to be and do as other freshmen, I even accepted compulsory attendance at the college chapel.

At the end of my freshman year, my mother died of a most painful cancer. This incident completed the destruction of any remaining orthodox belief in a beneficent Providence. That a gentle woman whose whole life had been the blameless and faithful discharge of her natural duties, involving constant self-sacrifice, patient self-control, and active effort on behalf of others, that such a woman should die such a death was an unforgivable outrage—if there were any personal and all-powerful Director of our destinies. The moral of it for me was that mankind must rely upon their own efforts to ameliorate their lot; prayer as a petition for help or protection from evil was a childish substitute for personal effort. Only scientific research could mitigate such horrors in the future. I was sobered and turned back from my boyish activities to more serious effort. I ceased to attend college chapel. When the Dean demanded an explanation, I told him my conscience would no longer allow me to participate; and he wisely let me go. I decided to take the medical degree and to specialize in physiology. Geology seemed to me a worked-out science; physiology was then in its prime at Cambridge and full of promise of indefinite progress. Besides, the medical degree would enable me, if necessary, to earn my bread and butter; and, given my father's capacity, several times demonstrated, for squandering a fortune in unproductive manufacturing enterprises, I could not count upon financial independence. At the same time I determined that a fellowship of my college after graduation was a very desirable step towards a life of intellectual activity and achievement. In those days, and I suppose still, a prize fellowship in a leading college carried a considerable prestige; and a fellowship of my college could be had only through success in a very severe competition.

So in my second year I buckled down to work, played few games, and found my recreation chiefly in long walks. For in those days the flat Cambridgeshire country, which now seems to me woefully dull and insipid, was still capable of ravishing me with its rural charm and beauty. I caught in those fields and marshes something of "the vision splendid" which now for me has faded "into the light of common day." Not the least of the pleasures of such a walk was the return to the beautiful old college where Wordsworth had dwelt and where I was often conscious that just beyond the college wall was the statue of Newton, "with his prim and silent face, marble index of a mind forever voyaging through strange seas of thought, alone."

It was in those days no longer the practice to publish the marks scored in the final or Tripos examinations; but it was usually possible to ascertain privately the marks of those nearest the top of the list, and I set out to score the highest mark in the natural sciences Tripos at the end of my second year. Since most men took the examination at the end of the third year, this would have been a considerable achievement. I did not succeed, but came very near to the success my self-esteem demanded.

I now had two years before me in which to specialize in physiology, anatomy, and anthropology, these being my chosen subjects for the second part of the Tripos, the part taken only by the relatively few serious and successful students. The first of the two years was one of comparative leisure. I had no examinations to pass and no compulsory classes. I read widely and became more nearly acquainted with the English poets. And I dabbled in metaphysics and ethics, deciding that here was a field worthy of my metal, in which all remained still to be done. In 1894 I passed the second part of the Tripos with the highest honors obtainable and secured the university scholarship at St. Thomas' Hospital, London.

Looking back at my eight years of undergraduate life, I feel that I specialized too early in biology. I should have been better equipped for a career in science if I had carried my mathematics to a higher point; and I regret that I did not obtain a wider acquaintance with the classical Greek and Latin authors at first hand. It seems to me now that these studies would have been more profitable than my early poring over fossils and petrological specimens. This must remain a question that cannot be answered. There is perhaps no man living who has had a more intensive and varied training in the natural sciences; and what intellectual faults and virtues I possess must be largely due to this long process of education through study of natural science. I suspect that to it I owe something uncompromising in my pursuit of truth, an incapacity to be content with one kind of truth in science, another in philosophy or religion.

When I hear condemnation of lectures and examinations and of the competitive motive in educational institutions, the memory of my own experience makes me inclined to defend all of these. There is little or nothing to be said in defense of compulsion to attend lectures. But a lecture system under which lecturers have to attract and hold their audiences by giving them something worth having is innocent of the evils against which diatribes are so often directed. The perpetual examination and "quizzing," so general in American colleges, are no part of an ideal system; but a final examination in which the student is called upon to make the best use of the knowledge he has been accumulating through several years is by no means a bad thing in itself; and the anticipation of such a test, with acceptance of responsibility for preparing for it, is, I think, good as a spur to intellectual and moral effort. As for the competitive element—man is not a creature ideally fitted for sustained intellectual effort; and, with very rare exceptions, if a man is to make the sustained effort which alone will develop his powers to the utmost, it is necessary that motives other than sheer intellectual curiosity shall be brought into play. After desire for understanding and knowledge, the desire of excellence, of self-improvement, ranks next. Closely allied is the desire to fit one's self for useful work in the world. The competitive system plays upon these motives; for it gives a man some measure of his progress towards these goals. And even the desire for public honors, for recognition as one who excels in some line of honorable effort, is not unworthy, and is perhaps the strongest of all motives upon which any system can make play. And that the student should be stimulated to intense effort seems to me the prime condition of preparation for an intellectual career. For myself, I can testify that I found profit in attendance at quite a number of lecture-courses, that I enjoyed the examinations and found them very stimulating, that all the motives mentioned above worked strongly, and that I was certainly not devoid of the desire for personal distinction. What proportion of motive power came from the last source I find it impossible to estimate; but I feel sure that it was no inconsiderable fraction. Especially the desire of the distinction of election to a fellowship of my college worked strongly within me. I was ambitious; but I looked down on all money-making vocations. All trade and business I regarded with mild contempt; and even the earning of a large income by the practice of law or medicine seemed to me unworthy of a free man.

It will be seen, then, that my youthful arrogance continued unabated. In deciding to take the medical degree of Cambridge University, it was not with the intention of practicing medicine; but rather I felt, as I still feel, that the course of medical study is a very desirable part of a thorough education, especially for one who aspires to work in any of the sciences concerned with man. There is no other way in which the student can bring himself into the most intimate touch with human nature in all its aspects. We see this in the effects of medical study. The men who enter upon it either fail to rise to its requirements and go to the dogs, or they become humanized, tolerant, understanding, sympathetic, and compassionate.

On going up to London in the fall of 1894, a further four years' course of study lay before me as preparation for the medical degree. I was not content to follow the regular courses in pathology, bacteriology, medicine, and surgery. I took also all the special courses available and undertook research in the Physiological Laboratory of the Hospital, then under the charge of C. S. Sherrington, who already was giving promise of the eminence since attained. I was fascinated by the problem of muscular contraction, and spent each long vacation at Cambridge and part of my time in London in seeking to provide a solid foundation for the hypothesis I had formed.

During these years at St. Thomas' Hospital I still led the double life, the bustling life of the medical student in wards and laboratories, and the life of the studious recluse. In the latter I had one companion only, Walter Myers, a sensitive intellectual Jew of my own age, who later succumbed to yellow fever in Brazil, after joining the first scientific expedition for the study of that scourge. The most important effect of my reading at this time came from William James' *Principles of Psychology.* I had, while still an undergraduate, determined that a life devoted to the study of the nervous system was the most desirable of all; for in the brain, it seemed to me, were locked the secrets of human nature. But James showed me that neurological research is not the only road to the uncovering of those secrets, and led me to believe that they should be approached from two sides, from below upwards by way of physiology and neurology, and from above downwards by way of psychology, philosophy, and the various human sciences. My plans were widened accordingly; and it was in accordance with the wider plan that I presented, in support of my candidacy for a fellowship at my Cambridge college, two theses, one embodying my physiological research on muscle, and one giving the results of my reflections on the psychophysical problem, an essay in which I foreshadowed the now fashionable doctrine of *emergence* of mind from the physical realm.

I was duly elected to a fellowship, much to my satisfaction. And I was not disturbed on being told that the judges who reported on both my theses expressed extremely divergent opinions. At that time I was more confident of my own powers and of the value of my work than I have since become—more arrogant, in short. I accepted the diversity of verdicts on my work as evidence that it was at least not commonplace and was above the level of those who reported adversely upon it.

During those years in London I was still a practicing disciple of Wordsworth. I had rooms looking onto the grounds of Westminster Abbey. The Thames embankment was my favorite walk; and often in summertime I saw the dawn break over the City from Waterloo Bridge and could say, "Earth has not anything to show more fair." Often, after a day in the laboratory, I would take the train into the heart of Surrey and walk over the downs, sometimes returning only at breakfast time. On one wild morning I jumped out of bed at four o'clock and caught the newspaper-train to Cornwall in order to see the storm break on the cliffs.

My election to a fellowship played a part in determining me against pursuing medicine as a career. I was in the running for the highest honors in that profession; and the career of a London specialist in neurology offered many attractions. But it seemed to me that the neurologist of that day did little more than achieve brilliant diagnoses of obscure organic disorders of the nervous system and prescribe mercury and potassium iodide with a vague hope of good results. The development of interest in the functional disorders, which now has reached so high a pitch, had hardly begun; and I had not the genius to foresee the great possibilities in that direction. I saw how difficult it is to follow medicine as a profession and to maintain at the same time an active interest in research; and I was all for research. Hence, when, during my time as interne in the hospital, I was invited to join the Cambridge Anthropological Expedition to the Torres Straits, I accepted with enthusiasm. The party was under the leadership of A. C. Haddon and W. H. R. Rivers, two of my Cambridge teachers whom I had learned to admire; and the task assigned to me was to help Rivers in making a complete survey of the sensory endowment of the negroid inhabitants of the islands. It was an opportunity to make intimate contact with a population of primitive culture; and I was already interested in such topics as totemism, exogamy, and primitive religion, having read Tylor, Lang, Frazer, and other authorities in that field. Further, I had become involved in a painful tragedy, the memory of which made me restless and ill-content with the life of cities and civilization.

We sailed from London early in 1899. My time in the islands of the Torres Straits was cut short by acceptance of an invitation from Dr. Charles Hose (then administrator of a very wild region in Borneo, part of the territory of the Rajah of Sarawak). He desired that some of the members of our party should spend a year with him, helping him to bring system and order into his prolonged and profound study of the many wild tribes of head-hunters, among whom he was establishing peace and prosperity. So I passed on from Torres Straits, after spending some five months in those remote islands, having greatly enjoyed the time, but having accomplished very little. In Borneo, where the people were even less touched by European civilization than those of the Torres Straits, I began the cooperation with Hose which continued until the publication, in 1912, of *The Pagan Tribes of Borneo,* a large two-volume work in which we dealt pretty thoroughly with all aspects of the lives of the very interesting and likeable tribes of the interior of Borneo. The very extensive and intimate knowledge of the people required for the writing of that work was, of course, supplied by Hose. My share, besides the actual writing, consisted in directing attention to problems on which new data were required, and in formulating hypotheses. In two of the latter I continue to find some satisfaction: first, the theory of the origin of totemism by way of the individual totem, an institution which, under the name *Nyarong,* we found flourishing among the Ibans or Sea Dayaks.

The second theory was that of the common origin and diffusion from a common center (in Asia north of the Himalaya) of the religion of the Kayans (one of the dominant tribes of the heart of Borneo) and of the religion of Ancient Rome. During my stay in Borneo I was strongly impressed with the similarities in general form and in certain details between these religions so widely separated in space and time. The theory of the essential oneness of the human mind seemed utterly inadequate to account for these similarities. On returning home I was able to find in the books on Roman religion more points of resemblance; and on publication of our article on the subject, Warde Fowler, a great authority on early Roman religion, was able to find others that had escaped me. I was thus a diffusionist before Eliot Smith's wide-ranging application of the principle of diffusion of culture-elements made the controversy—diffusion or independent origin—a central topic of contemporary anthropology.

After visiting China, Java, and India, where I learned to "hear the East a-calling," I returned to Cambridge. And now it was time to settle down to concentration along one line. In my desire to make as broad as possible my basis for the study of man, I was in danger of spending my life in excursions into the many possible fields. I was tempted to make field-anthropology my main line: for I greatly enjoyed wandering in wild places among primitive peoples and I had found it easy to make sympathetic contacts with such people. Looking back, I cannot now understand why I rejected this alluring prospect. I remember that my conscious ground of rejection was characteristically arrogant. I said to myself, "That field is too easy for me"; and turned back to my original scheme of direct attack on the secrets of human nature.

I read Wundt's books and found them very dusty. I read also Külpe, Ziehen, Münsterberg, Höffding, Bain, Hobhouse, Lloyd Morgan, Ward, Stout, and Lotze. Of all these authors, Stout and Lotze seemed to yield more nutriment than the others. Among all the German philosophical writers I had sampled, Lotze was the only one who stirred me to something like enthusiasm. I attended lectures by Henry Sidgwick and James Ward. I was certain that there was something very much at fault in contemporary psychology; but I could not define the fault. I decided I must make first-hand acquaintance with the psychology and psychologists of Europe. I inclined to visit Janet, Bernheim, Kraepelin, and Freud; but, under the advice of Ward (one of the very few instances in which I have accepted advice), I chose to sit under G. E. Müller at Göttingen, then the leading exponent of the exact laboratory methods in psychology. My choice was partly determined by what might seem an irrelevant consideration. I had, against my principles, fallen suddenly in love and become engaged to marry; and Göttingen promised to be a better scene for a year's honeymoon than Paris, Vienna, or other large city. We spent a delightful year in quaint, quiet Göttingen. My marriage at the comparatively early age of twenty-nine was against my considered principles; for I held that a man whose chosen business in life was to develop to the utmost his intellectual powers should not marry before forty, if at all. But nature was too strong for principles; and I have never regretted the step. It might be thought that for a charming young girl to marry an intellectual monstrosity like myself would be like making a bedfellow of a hedge-hog. But my wife has proved equal to the task she undertook. In intellect and temperament we were as unlike as possible, pure complementaries: I introverted, reserved, outwardly cold and arrogant, severely disciplined, absorbed in abstruse intellectualities; she extroverted, all warmth and sympathy and charm and intuitive understanding. To do one's duty by a wife and five children does require the expenditure of considerable time and energy that might *possibly* be given to purely intellectual tasks. But I have always found delight and recreation in my home; I have never ceased to grow more grateful to my wife for her influence upon me and her perfect exercise of the privileges of her position; and I realize that she has saved me from entanglements which, if I had followed my principle, might well have wrecked me. Then, too, I have learned more psychology from her intuitive understanding of persons than from any, perhaps all, of the great authors. I venture to think that the success of our marriage has been partly due to my recognition that the intellectual is apt to ruin his domestic relations by permitting himself to regard them as of less importance than his work. At a very early stage I resolved to avoid that error.

At Göttingen I followed Müller's lectures on psychophysics and on the experimental investigation of memory. They were admirably thorough and detailed. Yet I felt sure that these were not the main lines of progress for psychology. I was, then, not in close intellectual sympathy with Müller, though I admired his thoroughness, his energy and honesty and enthusiasm; and he and his wife treated us very graciously.

In my last year as an undergraduate at Cambridge, W. H. R. Rivers had entered on his duties as lecturer in the physiology of the sense-organs. He had recently returned from a long period of study under Kraepelin and Ewald Hering. Of the latter's theories he was an enthusiastic exponent. Those theories were in line with much of the work of W. H. Gaskell, whose lectures at Cambridge I had greatly appreciated. And those theories were then dominant in the physiological and psychological circles of Germany, England, and America. At first I was much inclined to agree. But I soon rebelled, and began independent experiments in the field of light- and color-vision, experiments which soon convinced me that Hering was on a wholly false line. I seemed also to see that his most fundamental physiological principles were wholly untenable.

This rebellion illustrates a tendency of my nature which has, I think, played a principal part in determining my lines of thought and work. It is allied to, but not wholly to be identified with, the arrogance which I have already mentioned. Whenever I have found a theory widely accepted in the scientific world, and especially when it has acquired something of the nature of a popular dogma among scientists, I have found myself repelled into skepticism. This tendency had already led me to espouse the cause of psychophysical interaction, as against the then popular and orthodox parallelism and epiphenomenalism. Now it led me to active rebellion against the dominant theories of Hering.

At Göttingen I carried on intensively my observations in the field of color-vision, finding the laboratory well equipped, owing to Müller's active interest in that field. I worked also on the development of a method for studying the problem of divided attention: for the singleness and limitation of the field of attention seemed to present problems of fundamental importance. Müller had written on both these topics, and, as usual, I found myself in opposition to his views.

At University College, London, James Sully had acquired, when Münsterberg went to Harvard, the apparatus which that distinguished pioneer had gathered in his laboratory at Freiburg. Sully's knowledge of the field of psychology was wide and deep, but he had not the least training for laboratory work. He desired to find a man to teach laboratory methods and had invited me to attempt this task. I undertook to give each year a short course of lecture-demonstrations, one meeting a week during one term only and at a nominal salary. In order to take up this work we returned to England and settled at the end of 1900 in a delightfully situated small house on the Surrey Downs near Haslemere. My very light teaching duties left me ample time for study. I read widely, especially in history, as preparation for an eventual *Social Psychology.* I also became interested in psychical research; and I wrote a number of long and careful reviews of important books, an excellent exercise for a young man. But my chief work of this, my most productive period, was experimental. I made a laboratory of two attic rooms in my house; and there during four years I carried on the most enjoyable and profitable of my experimental researches, mostly in the field of vision.

As I conceived it, I was carrying on my attack on the secrets of human nature along both the possible lines. I would burrow in from below by penetrating the nature of the retino-cerebral processes. I would at the same time approach those secrets from above by continuing to study the phenomena of attention.

These studies issued in a series of papers, all of which fell in the province of physiological psychology as I conceived it. I continued to hold the view of the psychophysical relation which I had suggested in my first paper of 1899, namely, the view that the psychical qualities are engendered by (or as would now be said, "emerge from") the complex conjunctions of brain-processes (now called "configurations") but not as mere epiphenomena, rather as synthetic wholes that react upon the physical events of the brain or have causal efficacy within the whole complex psychophysical event. About half of these papers were concerned with particular problems in the psychophysics of vision; the other half were more speculative and concerned with the general functioning of the brain, the synaptic functions, inhibition, and the phenomena of attention. I also found opportunities to study the phenomena of hypnosis and to see mental and nervous cases.

Most of my papers seemed to be still-born; but at that time I was not troubled by the fact. It was not that I was indifferent to recognition; but I had the naïve belief that sound and original work is sure of recognition in the long run. To some extent this belief was justified, for it was in the main the papers of this period that led at a later date to my election to the Royal Society. But there were only two or three persons in Great Britain interested in the special problems with which I was busy. German academic circles were hardly accessible to British contributions; those of America were dominated by the Germans; and, in both, Hering's views were orthodox. In the field of visual theory I had found it necessary to reject both Hering and Helmholtz (whose rival views had for more than a generation occupied the field) and to go back to Thomas Young. As regards the general functioning of the brain, I could not accept the view then and still now current among the physiologists, namely, that each neuron merely transmits to its neighbors a stimulus. It seemed to me clear that the beginning of all understanding of brain-functioning was to regard the brain as the seat of action of fields of energy, within which fields there was widespread reciprocal influence and free flow of energy from part to part. In both my main interests, then, I was as usual opposed to the popular or orthodox views. In consequence, most of my contributions of that period have remained buried in their original depositories.

During my teaching at University College, a little group of persons interested in psychology began to gather for informal discussions in my laboratory. After a time we made ourselves into a formally constituted group, the British Psychological Society, with, I think, twelve original members; and presently we held larger and more formal meetings in various centers, and undertook to publish a journal, the *British Journal of Psychology.*

At this time, also, or later, I became a member of many scientific societies, the Physiological, the Neurological, the Royal Anthropological, the Sociological, the Medico-Psychological, the Aristotelian, the Mind Association, the Royal Society of Medicine, the Society for Psychical Research, and the German Society for Experimental Psychology. Before all of these I read papers from time to time, and in most cases served on the governing body.

By 1904, when the Wilde Readership in Mental Philosophy at Oxford fell vacant, I had begun to realize that I was throwing my seed on stony ground, that my work along the lines I was pursuing could not find a public. I applied for the vacant post and was appointed. The post was in many ways an ideal one for me. The small salary was a welcome addition to my small income. The duties were very light—only two lectures in each of twenty-one weeks a year; and I was at liberty to choose my topics within a very wide field, a liberty of which I took full advantage. I ranged at large over the whole field of psychology conceived in the broadest way. I prepared my lectures with great diligence, writing out each one in full, and giving at least two full days' work to this task. During my tenure I must have prepared in this way at least thirty courses of lectures. This work was, I think, well worth while for me; though how much or how little my hearers profited I never knew. My classes were at first small, except when I lectured on such a sensational topic as hypnotism, with demonstrations; and then my large lecture room was crowded.

But the post had its drawbacks. It was, I think, T. H. Huxley who said that, if he had to devise a punishment for a very wicked scientist, he would condemn him to be a professor of science at Oxford. If I had been recognized as a teacher of science, my punishment would have been light; for by that date science was well established in Oxford. But I was neither fish, flesh, nor fowl. I was neither a scientist nor a philosopher *pur sang*. I fell between two stools. The scientists suspected me of being a metaphysician; and the philosophers regarded me as representing an impossible and non-existent branch of science. Psychology had no recognized place in the curricula and examinations. For some years I was not even a member of the University; for I could not become a member without first becoming a member of some college; and a man in my position could not, without indelicacy, ask any college to accept him. Further, I was annoyed by the efforts of the founder of the Readership to dislodge me. He was an old manufacturer who had a great admiration for John Locke and a conviction that the mental life cannot be experimentally studied; and he had learned that I had been guilty of efforts along that line.

Still, some of my colleagues were kind, especially the Professor of Physiology (Gotch), who provided me with a good set of rooms in his laboratory where, as a private activity distinct from my work as University Reader, I could carry on research. In these rooms I did both experimental research and teaching, always having a small group of special students, among whom were W. Brown, Cyril Burt, G. F. Flügel, M. Smith, M. Bickersteth, and Horace English. These and a few others I was able to regard with satisfaction as brands plucked from the burning and turned into the channels of productive research.

A large part of my time, the most delightful and not the least profitable to my professional studies, was spent with my children, on all of whom I made detailed notes during their earliest years.

In 1907 I wrote my *Social Psychology*, which, I imagine, will be reckoned my most original contribution to psychology. It was written by invitation as a member of a projected series of semi-popular scientific books, after the style of the old international series. The other members of the series never materialized. I had no thought that it might be used as a college textbook. I wrote for the general public. The genesis of the main thesis of that book is, I think, of some slight interest. Lecturing one day in 1906, I found myself making the sweeping assertion that the energy displayed in every human activity might in principle be traced back to some inborn disposition or instinct. When I returned home I reflected that this was a very sweeping generalization, one not to be found in any of the books; and that, if it was true, it was very important. I set to work to apply the principle in detail, becoming more and more convinced both of its truth and of its importance; and my *Social Psychology* emerged.

One of the greatest pleasures of my life fell in the year 1908, namely, a short visit from William James. I had never ceased to admire him greatly; an admiration which had increased when I met him for the first time in Rome in 1906. I felt that his visit was both a great compliment to me and a new evidence of the man's profound kindliness. During this visit James convinced me of the general validity of the pragmatic criterion of truth. Nevertheless, it seemed to me then, as it seems now, that James' pragmatism was not a philosophy nor a metaphysics (as so many have represented it), but just the extension to all fields of inquiry of the criterion long well established in the natural sciences. In 1910 I tried to express my appreciation of James in a short memoir contributed to the *Proceedings of the Society for Psychical Research.* James and Stout are the only two men of whom I have felt myself to be in some degree the disciple and humble pupil.

The psychophysical problem continued to fascinate me; and I turned next to a book on that topic, seeking to make it both a comprehensive survey of thought on the topic and a constructive contribution. I had become more and more convinced that the mechanistic biology was unsound; also that my early "emergent" treatment of psychical functions did not go far enough; that in all living things there is some factor which does not work in accordance with mechanistic principles and which has its own peculiar nature and organization. The works of Hans Driesch confirmed me in this view. Souls were out of fashion, as James had said. But I had a predilection for unfashionable doctrines. And, seeing that so many scientists seem to find satisfaction in shocking the bourgeois, I would shock them by putting up a defense of an exploded superstition. In this spirit of defiance I wrote my *Body and Mind* and gave it defiantly the subtitle, *A History and Defense of Animism* (1911). This, perhaps, is the most accentuated illustration of that uncompromising arrogance which I have already mentioned. The publication of this book, like that of my *Social Psychology,* was like dropping a stone into a bottomless pit. I waited to catch some reverberation; but in vain. Each book received, I think, one favorable mention in the press; and that was all. I never could discover that anyone in Oxford had read either of them. And my colleagues, with one or two exceptions, seemed to be shaking their heads very gravely.

About this time I began to find it difficult to believe in the value of my work, a difficulty that has grown steadily greater. I was much tempted to turn to medical practice before it should be too late. However, in 1912 I was elected a fellow of the Royal Society; and also, chiefly through the kindness of F. C. S. Schiller and of Thomas Case, the metaphysician who presided over Corpus Christi College, a fellow of that College. My position was thus greatly strengthened; and I felt a certain obligation to persevere in the paths of pure science, however little I might effect.

During all these years I had been working sporadically on *The Pagan Tribes of Borneo.* Hose was now retired and living in England. In 1911 we spent the summer together and finished the book. This was largely a labor of gratitude on my part, for whatever kudos the book might bring would naturally and properly go to him.

In 1912 I was invited to write a small volume for the Home University Library, and produced my *Psychology, the Study of Behavior.* It embodied in small compass a good deal of hard thinking. In it I sketched very briefly the scheme of the psychology developed in more detail in my *Outline of Psychology* (1923). Although the little book was a very difficult one for the general reader, it has had a very considerable circulation, running somewhere near 100,000 copies.

During the ten years at Oxford before the War, I carried on work in the laboratory continuously, publishing a few experimental papers. But much of it was unfinished at the outbreak of war and remains unpublished. Among other things I was concerned to devise a series of mental tests that should be, as far as possible, independent of language and of learning, and universally applicable. And I worked especially on the influence of drugs on the brain functions. I was also preparing notes for a work which I projected as my *magnum opus,* a series of volumes on Social Psychology.

Shortly before the War, also, I had become much interested in psychoanalysis and, having met C. G. Jung in London, had made arrangements to visit him at Zurich in order to be analyzed by him.

But the War came, and I found myself a private in a French army, driving an ambulance and dodging German shells on the western front. Early in 1915 the British War Office began to realize the extent of its task, and there was a grave shortage of medical officers. I offered myself, was made a Major in the Royal Army Medical Corps, and was put at once in charge of nervous patients. At this time there was a flood of mental and nervous cases streaming home from the armies on all fronts, and there was little preparation for dealing with them. But it soon became clear that the "shell-shock" cases required mental treatment. I was put in a position where I could select from this vast stream whatever cases seemed most susceptible to treatment. And soon I was the head of a hospital-section full of "shell-shock" cases, a most strange, wonderful, and pitiful collection of nervously disordered soldiers, mostly purely functional. One had little time to think out the many theoretical problems. One thing was clear—successful treatment required the exploration and fullest possible laying bare of the causes of the trouble. Hypnosis proved very useful as a method of exploration, but not always indicated or feasible. Sympathetic rapport with the patient was the main thing, not a mysterious "transference" of a mythical "father-fixation" of the "libido"; but, under the circumstances, a very natural and simple human relation. It is true that I felt like the father of a multitude of helpless children, hopelessly stumbling on the brink of hell; and that they for the most part were very docile and dependent and grateful. It was a wonderful experience for a psychologist; and besides, for the first time in my life, except for my short period as house-physician at St. Thomas' Hospital, I was giving my whole time and energy to work that was indisputably worth while.

This medical work occupied all my time until the middle of 1919, when I returned to my university work; continuing, however, some psychotherapeutic work in a new branch of the out-patient department of the Oxford City Hospital.

During the War I had lost my laboratory, which was occupied by students of aviation problems; and after the War the rush of students to the Physiological Department made it impossible for the Department to return the rooms to me. It would have been most natural to devote myself to writing up my observations (filling thirty-five note-books) on nervous disorders. But I felt that I needed to digest them in the light of a more thorough study of the literature than I had then made. I felt also that it was very desirable to carry out my plan of submitting myself to psychoanalysis at the hands of an expert. I, therefore, found opportunities to visit C. G. Jung at Zurich and to be analyzed, so far as that process is possible for so hopelessly normal a personality as mine. I made an effort to be as open-minded as possible; and came away enlightened but not convinced. Before the War I had gathered a mass of notes for a book on Collective Psychology; and I felt that, if I did not at once work these into a book, the task would never be done. I was forty-eight, and though my father's family was long-lived, my mother's was less so; and physically I belonged to my mother's side. Therefore, I wrote my *Group Mind*. The title was unfortunate, for it antagonized many; but the thought I sought to express in this title was sound, namely, that a highly organized enduring group, such as a true nation, possesses an organization which in the main is mental; an organization which resides not in any one individual but rather is only very partially resident in any one member of the group; and which is what would now be called a configuration or Gestalt, an organized system of interacting energies, every part of which acts only through and under the influence of the whole. My *Social Psychology* had been meant not as an introduction to the field, but rather as an indispensable preparation or propaedeutic. *The Group Mind* was a part of my projected *magnum opus;* but its reception was so unfavorable that the *magnum opus* went a-glimmering. For, as I have said, I have found it increasingly difficult to believe in the value of my work.

Then came the invitation to Harvard. It was in every way a very flattering one. The Chair of Psychology at Harvard had not been filled since Münsterberg's death during the War. The tenure of it by James and Münsterberg and the great prestige of the Department of Philosophy and Psychology seemed to justify me in regarding it as the premier post in America, where psychology was so actively cultivated.

I had always felt the lure of life in America as a land of romantic possibilities; New England and its history had fascinated me. A visit and a most friendly reception in 1913 had in part confirmed my impressions. Especially I was attracted by the way America seemed to experiment, to act, to put things through on a large scale. Then, though I had inherited, on the death of my father in 1914, enough to make me modestly independent under pre-war conditions, prices were doubling, the income-tax took a third of my income, my children were at the most expensive age, and England was beggared.

On the other hand, I had a secure and comfortable position at Oxford in which I could live out my working years; and after the War there was a marked increase of interest in psychology; my regular lectures now had some two hundred hearers.

However, it had always been my principle to accept whatever challenge life might bring. Harvard would be a stimulating adventure; whereas at Oxford I might too easily subside into inactivity.

The motivation in such a decision is vastly complex. One factor was, I think, that in spite of the extreme and unremitting kindness of Sir William Osler, we had recently lost a child from rheumatic fever; and I was savage against the English climate, which also I blamed for total deafness in one ear. I was inclined to settle the question by the toss of a coin. I accepted and put myself in full harness for the first time in my forty-ninth year.

We went to America with good hopes and intentions. I knew my wife and children would make themselves much liked; and I was determined that, as far as in me lay, I would represent my country creditably, at least with unfailing good-will and courtesy.

My amiable anticipations were a little checked when my arrival coincided with a stinging and very hostile review of my *Group Mind*. I found Behaviorism ascendant and rampant. I found that, though my *Social Psychology* had enjoyed before the War a much larger vogue than I had realized, it and I were now back-numbers, relics of a bygone and superseded age. I had undertaken to give a course of Lowell lectures in my first year, and I incautiously lectured on national eugenics. The lectures, published as *National Welfare and National Decay* in England, were slightly altered to give them American application in the American edition, under the title, *Is America Safe for Democracy?* I did not then realize that in touching, however impartially, the racial question, I was stirring up a hornets' nest. To this raising of the racial question in 1921 is due, I must suppose, much of the hostility of the American press that has continued to greet my successive publications.

Another difficulty which I had not foreseen was that the numerous graduate students were drawn to Harvard in the main from other colleges and universities; and, with very few exceptions, they had been taught some form of mechanistic psychology, with the consequence that they looked upon me and my outlandish theories with suspicion, a suspicion which yielded, if at all, to a more receptive attitude only about the end of their period of study at Harvard.

In spite of these drawbacks, we have been very happy in America. My colleagues were perfectly genial; we have found many dear friends; and I have never regretted our adventure.

I was not director of the laboratory; and my favorite field of experiment, that of visual perception, was already filled by a colleague. There was vacant a small equipment for animal psychology. I eagerly seized the opportunity to begin an experiment I had long contemplated. In my Cambridge days I had rebelled as usual against the then all-dominant neo-Darwinism or Weissmanism. It seemed to me that the only ground of the dogmatic rejection of the Lamarckian theory was purely a deduction from the mechanistic dogma in biology; and I had urged that some strong scientific society should initiate and maintain, in a way not possible to any individual, a prolonged experiment designed to settle the Lamarckian question once for all, using preferably dogs as the most likely material. Now, in 1920, the question seemed just as open as in 1890, and no nearer a decision. Meanwhile, I had become more firmly convinced that the mechanistic dogma is no valid basis for biological deductions.

It was clear that, if I should use dogs as my material, I could not hope to live long enough to carry the experiment to a conclusion. I chose, therefore, the white rat which, in addition to so many other advantageous features, breeds rapidly. So, with a small group of graduate students, I set out on this fool's experiment. Yet not altogether foolishly, for, even though the issue might be entirely adverse to the Lamarckian hypothesis, a clear-cut negative issue of a well-planned and long-continued experiment would be not altogether without value; since no such experiment had been made. And, in any case, the question at issue seemed to me the most important question yet formulated by the mind of man and clearly susceptible of solution by experimental procedure. And a positive answer indisputably established by experiment would not only give us a working theory of biological evolution, but would be a heavy blow to the mechanistic biology. It would place mind at the very heart of the evolutionary process, instead of leaving it as a by-product of that process, an unintelligible excrescence upon life.

The experiment, now in its tenth year and its twenty-forth generation, seems to promise a clean-cut and indisputable proof of the reality of Lamarckian transmission. If, in the next few years, this promise should be amply realized, the work will rank as by far my most important contribution to science; although the execution of it will have required little but great confidence in my own judgment and dogged persistence. This work has absorbed all the time and energy I have had for experimental research.

By devoting my long vacations to writing in a retreat among the beautiful White Mountains, I managed, in spite of the long and arduous academic year at Harvard, to produce several books, as well as many articles. Of these the largest and most important, as I suppose, are my *Outlines of Psychology* and of *Abnormal Psychology*. The latter was the topic to which I felt I could make a definite contribution. But I had before me as a warning several distinguished examples of men who, while making great contributions to that field and through it to psychology in general, had gone widely astray through plunging into it without having first acquired a foundation in the shape of a consistent and workable general psychology. And I could find no book that would serve me satisfactorily as a text for a course of instruction in the general principles of psychology. Therefore, I wrote first my *Outline of Psychology*, developing as systematically as I could the principles briefly laid down in my

Social Psychology and my little volume of 1912. It embodies a scheme, more complete, I think, than any other, of the general structure or organization of the mind and of the development of that structure from its innate basis.

I had come to see more and more clearly that the main defect of the psychologies with which I had struggled in the opening years of the century was their acceptance, or their compromise with, the mechanistic biology, and their consequent neglect of the purposive or teleological aspect of all mental life. I seemed to see clearly that, whatever theory of the relation of mind to matter (of the psychophysical relation) one might hold, any psychology that ignored, or failed to bring out clearly, the fundamentally purposive nature of mental activity was doomed to sterility. It seemed clear also that that kind of purposiveness which is involved in the hedonist, the pleasure-pain, theory of motivation was utterly inadequate and misleading, especially when, as by so many of the nineteenth-century authors, it was clumsily attached to an otherwise thoroughly mechanistic associationism. The most essential character of life-processes seemed to be their goal-seeking nature. But goal-seeking is the type of activity we find most clearly displayed for our contemplation in all our own most developed activities. It seems, then, likely that where there is life there is mind, or, at least, that form of goal-seeking activity which becomes what we call mind in highly developed organisms. A word was needed to express this type of activity in the most general way; and the Greek word *horme* seemed the only one available. So, putting aside the psychophysical problem, leaving it an open question, I wrote a hormic psychology, of which the keynote is the hormic urge to live, differentiated in the course of biological evolution into the specialized forms that we call instincts. The innate basis of the human mind was thus for me not merely certain reflexes (mechanical even though called instincts in the more complex instances) together with a capacity for certain qualitities of sensation. I could not follow Lloyd Morgan and his many disciples in supposing that by adding sensations and images to mechanical reflexes we can generate intelligent mental activity. Sensations were to me unreal abstractions; and "ideas" and "concepts" were anathema, the fountain-head of most of the confusion in modern philosophy and psychology. The mind was in some sense a unity from the beginning and developed not by accretion of sensations, images, and ideas, but by a process of perpetual differentiation and specialization of its rudimentary powers of knowing, of feeling, of striving towards goals. Psychology must begin by recognizing frankly the peculiar nature of the facts it deals with, and must postpone, indefinitely if need be, the task of reconciling itself with the sciences of the inorganic world.

Bergson seemed to me to have established the radical difference between habit and true memory. Habit is a matter of connections between neurons; but whatever may be the foundation of memory, it is a continuously growing organization of a nature distinct from the neural basis of habit, yet functioning in intimate cooperation with that basis.

Rudimentary knowing, feeling, and striving are given in the innate basis, as functions of the relatively simple innate structure of the mind. Psychology, at present at least, must be content to accept these functions as primary postulates, its task being to describe the differentiation of them through the growth and differentiation of mental structure. Alongside the process of increasing differentiation of structure and functions that produces the organized intellect, goes on a process of increasing integration, the process of character formation.

The attempt made in my *Social Psychology* to define the innate basis of the mind in the human species had suffered a curious fate. It had been adopted *en bloc* by many authors, and cited by many more in a non-committal way; but hardly any serious criticism or attempt to correct or improve upon it had been made, though a multitude of superior persons had jeered at it. In America I was known as a writer who had flourished in the later middle ages and had written out a list of alleged instincts of the human species. Yet it was certain that my attempt, if it was an approximation to the truth, was very important. The psychoanalysts, who, like myself, founded all human activity on a basis of instincts, were floundering wildly through lack of any comparative study of instinct, any attempt to define what instinct is and does and can do, and what instinctive tendencies are proper to man. I could not see that in the fifteen years elapsed since my *Social Psychology* any progress had been made with this fundamental problem. Nor could I find that my scheme needed radical alteration; it seemed to require only improvement in detail and completion.

On the basis provided by my *Outline,* I built up my *Abnormal Psychology,* incorporating what seemed most sound in the teachings of Freud, and Jung, and Morton Prince, especially the principles of conflict, repression and dissociation, and the subconsciously working complex.

In my *Social Psychology* the functional units of mental life were described as sentiments, each sentiment being regarded as a structural system comprising all knowledge of and all affective tendencies directed upon some object; and the formation of character was the integration of the sentiments in one balanced self-consciously operative system. That such functional units are very real was obvious; but the great majority of psychologists had remained content to use the vague notion of "the complex" (defined as a repressed emotionally toned idea) or the still vaguer term "attitude."

This scheme of the mind's general structure and modes of functioning could, of course, at best be only approximately true; but I found strong support for it in the fact that it lent itself readily to the interpretation of the whole range of functional disorders. And, in fact, the only serious objection hitherto offered is just the fact that it does work so smoothly and neatly. It seems to be felt in many quarters that in psychology a working hypothesis that works is an anomaly and something of a monstrosity.

In my *Abnormal Psychology* I endeavored to develop as a scientific hypothesis the monadic theory of human nature. For this seems to me the only view capable of reconciling the facts of the unity of consciousness with the facts of disintegration of personality, of multiple consciousness, and of relatively independent subconscious mental activities. The theory, of course, raises many difficulties; but that is true of every possible psychophysical theory. My aim, as in my *Body and Mind,* was to bring this problem down from the airy region of metaphysical speculation onto the plane of science and scientific method.

Since publishing my *Abnormal Psychology* in 1926 I have made one further step in my own thinking, embodied in a small volume, *Modern Materialism.* This volume aims to establish the reality of purposive action as a form of causal efficacy distinct in nature from all mechanistic causation. It points out that, in spite of all the contemporary talk of the reconciliation of Science with Religion, no such reconciliation has been effected or can be effected so long as biological science remains essentially mechanistic, recognizing only mechanistic explanation, namely, explanation through events, however immaterial, that lie wholly in the past, explanation that makes no reference to the future. For biological processes are teleological, they require teleological explanations, explanations which refer to ends or goals; and, so far as we can see, such reference can be intelligibly conceived only as mental reference. Hence foresight of goals, stirring to action and directing the hormic energies of the organism, would seem to have real causal efficacy in the life of the organisms.

The admission of the reality of such teleological causation, an admission that Science has not yet made, seems to me essential to all religion and all morals. To reconcile Science with morals seems to me a more urgent need than its reconciliation with Religion. I have never yet been able to convince myself that religious belief of any kind is an imperative human need. And I cannot conceal from myself the fact that religious belief has been and is now the ground of much dishonesty, that it becomes increasingly difficult to hold and profess such belief without dishonesty. On the other hand, belief in the efficacy of moral effort and in the reality of moral choice does seem to me an imperative human need. Without it, we are discouraged, paralyzed, and thrown back, individually and socially, into moral chaos. The Mechanistic Science that is still dominant does deny us such belief. Hence, if such science is in error, the importance of speedily convicting it of error.

When I use the words "religious belief" I mean belief in some theocratic governance of the world. I know that it has long been customary to apply the term to beliefs about the world which are not in any sense theocratic. But that seems to me one instance of the dishonesty into which the prestige of religion is apt to betray good men. A system of belief that contains no theocratic element cannot properly and honestly be called religious. To give it that title is to deceive and to deceive intentionally. Religion has been defined as "the art and theory of the internal life of man." But this art and theory are the art and theory of morals, not of religion. And though, no doubt, such art and theory have been the concern of all churches and of all the higher religions, they are no essential part of religion. When I say, then, that I doubt if religious belief is an imperative need of mankind, I mean theocratic belief. I do not doubt that, if we could see good grounds for accepting such theocratic belief as William James inclined to, such belief would be of moral value. The mystical experience of the few who attain to it seems to suffice for their conviction. But I see no way in which such experiences can be made evidential for the rest of us. And, apart from such experience, the desire of theocratic belief seems to be the only ground of it. I am still prepared to believe that the Christian religion may be in essence true; but I still see no sufficient ground for such belief, though in a vague way I share the desire. The desire of belief in theocracy, however universal, is, to my mind, no sufficient ground. If it proceeds from lack of courage to stand alone in the world, it seems to me of no great merit. If it is a form of benevolent desire to see mankind rendered happier by such belief and encouraged in right doing, it has some moral justification. Yet if it leads us to distort the evidence, to blind ourselves to any part of it, to weigh it with less than the strictest honesty, such desire and such belief are morally stultified.

What then of my dabbling in *Psychical Research?* What is my apology for such pandering to superstition"? It is probably true that the majority of those who have taken an active interest in this field have done so in the hope of providing surer foundations for religious beliefs, especially for the belief in the continuance of personality after the death of the body. I was led to make some study of this field by my desire to know the truth. Here, it seemed to me, was a body of ancient beliefs all of which Science seemed utterly to deny. Yet the ground of such denial was plainly inadequate. It was in the main an inference from the assumption that the universe is a strictly mechanical system. Here were phenomena alleged to occur in all times and places, an allegation supported by a body of strong testimony. And Science frowned upon it all and said: "Such things cannot happen." As usual I was thrown into rebellion against this orthodoxy. Further, I saw in the Society for Psychical Research a body of earnest seekers after truth, conscientiously using methods which might reveal truth; and these researches were largely in the field of psychology. Yet not only scientists in general, the philosophers, the churches, and the men in the street, stood coldly aloof or actively scoffed, but also the psychologists. And it seemed to me a scandal that psychologists should refuse to lend a hand or at least moral support to this heroic effort. Therefore, though without much hope or anticipation that any phenomenon (beyond those that fall under the head of telepathy) would be established, I threw myself to the support of Psychical Research. I felt that even a purely negative result of a long sustained cooperative research would be of great importance. For, until such research shall have been made and shown to be incapable of finding any basis of reality in the alleged supernormal phenomena, the world must continue divided into ignorant partisans and ignorant deniers.

It is peculiarly difficult to maintain a strictly scientific and impartial attitude in this sphere; a fact illustrated by the very small number of persons who have succeeded in doing so. It is difficult to avoid the influence of the confidence of the scientific world in the adequacy of its own principles, without falling under the contrary influence of traditional religion. But also a positive temptation of a very real nature besets the inquirer into these obscure questions; especially, if he has any reputation to lose or to throw into one or other scale. If, on investigating some notorious case that has excited popular interest, he hastily and roundly denounces it as purely fraudulent, he earns the applause of one half of the world; but, as I know from my own early experiences of such sensational "exposures," he does little or nothing to clarify the field. If, on the other hand, he affirms its genuineness as an instance of supernormal happening, he wins the plaudits of the other half of the world and is accepted as a shining light among them. But if he devotes much careful study to it, and renders a judicial report, balancing carefully the pros and cons, then he becomes to both parties an object of vituperation and contempt. Although the last fate has been mine, I nevertheless find a certain satisfaction in having maintained the scientific attitude of impartial inquiry in spite of all difficulties and unpleasant consequences.

I have served on the council of the English Society for many years. I have presided over it and over the American Society, and have taken an active part in founding the new Boston Society. And, though my contacts with the field in America have brought many very disagreeable incidents, I do not repent. I have given the minimum of support which, as a psychologist occupying a position of some slight influence, I could give without reproaching myself with cowardice. If I had not found it necessary to earn some income, I should perhaps have chosen to give all my time and energy to work in this field. During my thirty years of Psychical Research I have grown rather more skeptical of the "physical prenomena" (though even now I am not prepared to assert that they do not occur) and more inclined to believe in the reality not only of telepathy but also of some of the other "mental phenomena."

What of my attitude toward Philosophy? I have associated much with philosophers and have read philosophy all my life. It has become increasingly clear to me that there is no method (call it metaphysical or what you will) distinct from the scientific method for the ascertainment of fact and of the nature of reality. I am convinced that the scientific or pragmatic criterion of truth is the only valid one. Yet I do not, therefore, like many men of science, deny the right of Philosophy to existence as an independent discipline. I deny that it has any right to attempt, or any method for, the building up of cosmologies or ontologies. But I recognize that Philosophy has a large field of its own which must forever remain outside the province of Science, namely, the field of valuation, the investigation of values and of judgments and standards of value. But I recognize also that, in all this important work, Philosophy has been hitherto very seriously hampered by the lack of sound psychology. And because psychology stands in this relation to all the philosophical inquiries (and also to all the sciences of man and society, a relation far more intimate than that of the other sciences), I still hold, as I held in my youth, that it is the science of most urgent importance in the present age, when, for lack of sufficient knowledge of human nature, our civilization threatens to fall into chaos and decay.

I am, then, interested in "the art and theory of the internal life of man." It is, I think, my fundamental interest, that which has led me to devote such powers as I possess to the study of psychology. For the art and theory of the internal life of man have inevitably remained rudimentary and highly disputable for lack of a foundation of scientific knowledge of man. If I thought that psychology were incapable of furnishing the required foundation, I should not regard it as of much interest, and should not have given my time to it. It is the most difficult of the sciences, and the most unsatisfactory of all fields of research. It is far too difficult for most of us who are engaged in it; and I see no clear prospect of steady advance. To hardly any major question within its field is it possible by any method to find an answer which shall compel the assent of all qualified students. We shall continue to stumble along, divided into warring sects, accumulating vast masses of data, but unable to interpret them in terms of any one comprehensive and generally acceptable scheme. But somehow, no doubt, and however slowly, progress will be made.

When I entered the field of psychology it was a field for specialists alone, and in my own country such specialists were very few, though in America and Germany their number was rapidly growing. At the present time psychology is not only strongly represented in most universities (outside of Great Britain), but has secured pretty general recognition in the fields of Education and Medicine. It has also attracted the attention of a very considerable part of the general public. It may even be claimed that psychology is now the most popular of the sciences. But I find no great encouragement in the last fact. In many ways the popular interest in psychology is a disturbing and distorting influence, especially in that it gives an undue prominence and prestige to views that are extreme, ill-balanced, fantastic, and bizarre, if only they contain some modicum of truth and are put forward with persuasive skill. In America, especially, the general public, including not merely the seekers after personal benefits but also the more cultivated public, is keenly interested in the extravagances of the Freudian school, in the equally ill-balanced system of Adler with its gross exaggeration of one factor of our constitution to the neglect of all else, and in the still more ill-balanced, extravagant, and bizarre dogmas of the behaviorist school, in the equally inadequate and lopsided doctrines of Couéism and of Christian Science, and in the sensational claims of the Spiritists. On the other hand, it ignores the labors of those who try to maintain and, by patient research, to develop a sane, all-round, well-balanced system of psychology that founds itself on general biology and takes account of facts revealed by all relevant lines of research, by biology, by physiology, by anthropology, by the study of animal behavior, by the medical and the social sciences, by "psychic research." For the general public such psychology is too difficult, too laborious, too lacking in sensational claims, in promises of immediate solutions of practical problems, too humdrum, too tame, too full of unverified hypotheses and confessions of ignorance. What the public likes is to be told straightforwardly and dogmatically that it has an Unconscious, source of all mysteries and all solutions; or a terrible Oedipus Complex, source of all disorders; or an Inferiority Complex, source of all achievement; or a few Conditioned Reflexes that explain all human activity; or a miracle-working power of Auto-Suggestion; or an Etheric body; or an imperishable Soul. And whatever the dogma, it must be one that promises immediate profits in health, or pocketbook, or domestic harmony and relief from personal responsibility.

Of all the great problems that confront the psychologist none seems to me so urgent as that of the nature and extent of the innate basis of our mental life. As regards both our intellectual and our moral nature, the question remains obscure; and there is room for wide differences of opinion. I have given much thought to it, but can see no method for its sure elucidation; for the experiments that would throw a clear light upon it are barred by moral considerations.

The problem of the relation of mind to body is equally obscure, and, in principle, even more baffling. Here the methods of psychical research may possibly bring light. The man trained in science finds it difficult to accept freedom of the will, telepathy, or survival of personality after death. Yet these may be in some sense intelligible; the intellect may reconcile them with the rest of scientific knowledge. But, when we are confronted with what seems good evidence of clairvoyance or of prevision, we are simply nonplussed. In every direction we seem beset with impenetrable barriers against which we dash ourselves in vain.

Do I then regret the choice of my line of work? Sometimes I do. Similar abilities, energy, and sustained effort, applied in any other line of work, might well have brought considerable reward. Some of my books have been moderately successful in finding readers among the general public. But if, as the majority of my colleagues say or imply, they are utterly misleading, what a weight of responsibility lies at my door in having misled so many innocent readers! I sometimes contrast my work with that of William James, my model. It is on record that, within a few weeks after the publication of one of his less popular books, he received letters about it from some five hundred persons. Whereas, if I receive from those to whom I have sent copies of a newly published book three or four postal cards and a couple of letters, I feel that I have done pretty well. The more I write, the more antagonism I seem to provoke. Yet, except in one or two reviews written when I was still a green hand, I have been at much pains to be strictly fair to those with whom I do not agree. I have not been able to acquire James' magic touch which made all his readers his friends. I suppose it is that my uncompromising arrogance shows through, in spite of the taming it has undergone.

Yet in the main I have lived hitherto the sort of life which in my youth I judged to be the most desirable; and that perhaps is all a man can properly demand. Even if my books are very much at fault, many of their readers may have profited in some degree from the intellectual effort to comprehend them. I have done no great wrongs; and, as I often tell myself, it is something to have done my part in bringing up a little flock of whom I may justly be proud. And yet, was it right to bring them into existence? Was the Buddha's teaching true? It is a deep question, and I have found no answer.

CARL EMIL SEASHORE

I. Heredity and Education

"What are you doing, my little boy?" said the village policeman to the two-and-a-half-year-old urchin playing on the rune-covered flagstones of the front steps of his home in a Swedish village.

"I am looking for mischief," was the prompt reply. Since this is the earliest record of my self-evaluation extant, it may be appropriate to enter it as the first item in my autobiography.

The remark may have been a spontaneous taunt to the policeman and his job, as much as to say, "I am your victim." It had the earmarks of an original observation. However, even at that, it was probably not original; but an echo of Mother's oft-expressed opinion, "Carl, you are always getting into things," a sentiment I had accepted in good faith and, therefore, held as my own.

After all, was not this remark of a child, in itself a characteristic reaction, true not only of the child, but also of the man to be—adaptive, responsive, venturesome? And, is it not likely that the ideas that will be set forth in this paper as a result of self-analysis of the mature man may be in reality the echoes of other persons whose opinions are held in high esteem? As a social reaction, it is certainly true in the rating of our faults; it is probably equally true in the appraisal of our fortes. Our checks are as good as our credit.

Many years ago, while writing on the nature of imitation, I asked myself, "Is there anything in my exhibition of individuality which is not ultimately traceable to some other personality?" Deviants of all sorts—smile and gait, arrogance and modesty, self-restraint and greediness—hundreds of traits were reviewed and accounted for in the negative. But finally, glancing at my notes on these things, there flashed a thought, "Here is something peculiarly my own." It was an abandon in a superfluous flourish in the loops of my small *y* and *g*. There proved to be a constant form of these loops, and I recalled it as a frequent source of a feeling of individuality in my handwriting—an exhibition of myself. No one that I knew had indulged in, or hit upon, that exhibition of personal fancy—not much of personality, but the lone instance of an original element that I had teased out so far.

However, as a psychologist, I felt that the strength of the evidence must be proportional to the strangeness of the event. So I decided to examine all the available traces of handwriting of my friends. Among other things, I hauled out all personal letters that had been saved, thinking that I might there find the root of this sport growth. Sure enough, to my surprise, I found it in some letters from a young lady whom I had greatly admired, platonically. I say platonically, for this we had agreed upon, because she had another sweetheart and so had I. Kindred souls, we had been stranded together for a year in a small village when I was twenty-one. Every letter of hers showed the perfect original, of which my puppet copy was but a poor struggling imitation. "There," I said, "love is blind," for, although I had seen her handwriting daily in professional associations and had read those letters for a year, I could not recall having noted this feature as such. A splendid example of imitation it proved to be, for I had unconsciously taken over a trait of a person I admired.

It is not my nature, human nature, to imitate a sport trait of a woman. That would be effeminate, unworthy of a man. I had admired the girl, not the Gibson lines in her profile, and this loop was a Gibson line. I am now confident that a close observer would have seen in me many other glittering reflections of her "personality," mere lines in themselves, but essential to the true picture of her and of myself.

Is not that the law of imitation in the building of all personality? We grow to be like those whom we approve, all the meantime struggling to be ourselves. It has often been remarked that my wife and I resemble each other. Certainly I have grown to resemble my wife, and the process is probably reciprocal.

This reflection makes the writing of the story of my life difficult for it confronts me with two bewildering situations: first, the realization that I am essentially an indescribably complex bundle of elements borrowed from the personalities of others in my social environment from infancy to the present moment; and, secondly, swamped and overawed by feelings of indebtedness to all of these, I am helpless in giving credit to whom credit is due.

However, granting all of this subconscious absorption from the environment, am I not on the good side, the result of profoundly conscious, even emotional struggles to attain ideals I cherish? Graham McNamee recently said, "Our fundamental aims in education used to be the three R's; now we have a new ideal. It is the three C's: character, culture, and citizenship." True, individual development, like social evolution, is now more consciously directed than ever before; but these conscious accretions represent in large part what we wish to seem to be rather than what we are. In the main, we are but the constantly evolving flux of those traits in our personal environments which are associated into the original but constantly changing matrix of personality, growing mainly through subconscious accretions.

I have been a lucky man—lucky in the place and race of my nativity, in the "choice" of my parents, in my education, in my jobs, in my travels, in my marriage and children, in success and recognition beyond my fondest expectations. Whether this luck is real or just a point of view, others must judge. I mention it here, because as I attempt to list the items of my life that may deserve mention, the credit side overbalances the debit. Lest the reader may say that the picture is untrue, I venture the view that it is colored by luck. Things have come my way. At least, I have always thought that they have. Luck is, after all, a phase of optimism, and I am somewhat of an optimist.

Luck is a relative matter. In my adolescent years, I faced a summer of dreaded dullness. I was to teach country school in a rural backward district. The first morning I stuck this motto in the mirror of my room: "Blessed is he that expecteth little, for he is not to be disappointed." It turned out to be one of my happiest summers. Teaching was a delight. At four o'clock I took a basket with Gray's *Botany* and, carried by a companionable pony, I collected flowers from woods and prairies. I made friends in the community; some of them among my most cherished friendships. Life was so full of just what I wanted that I counted myself lucky. But looking back at that picture, I now see the wisdom of my motto, a version of my sister's, saying: "If it does not go very well, it will go very well anyhow." At least, the luck was enhanced by contrast with my anticipations or demands.

Luck is living on a rising scale. That is American. Be it bane or blessing, it is the gift of pioneer life. In a small way I have had the luck of pioneering. I grew up in the Central West. The Iowa farm boys will, perhaps, never again live on such a rising scale as we of my generation did. The opening of Nature's enchanted vistas by a new science of psychology is perhaps difficult to parallel in any other science. This luck raises me to the level of a pioneer in psychology, also. Perhaps, my largest flights into new territory have been in the field of leadership in higher education. The reorganization of education on the new foundations of psychology moves like an avalanche. In all of these, I have lived on a rising scale. That is the joy of it. It is realization of legitimate but unearned increments. In all this climbing I have encountered hardships, disappointments, and disillusions; but these fade out of the view as I regard the movement from vantage ground to vantage ground on a rising plane.

In this respect my life has been full of happy surprises. The immigrant boy, facing the hardships of the Iowa prairie, found opportunity blossoming everywhere in the rise of the material comforts of that frontier. In education my three brothers and I were pioneers. Coming from a community in which college education was a brand-new idea, all four of us obtained a doctorate or two, a rare event in pioneer farm life. It came to all of us on a rising scale. To quote from a paragraph in my "Open Letter to Seniors":

"To most of us the high places seem distant and beyond reach. In my own personal experience this was strikingly so. I took one year of a preparatory course for teaching in my own country school. I took another year to prepare a little better. I took three more years to prepare for teaching in a city school. I took another year to prepare better. I took another year to prepare for teaching in the university. I took two years to prepare better. Thus, from the moment I left the plow to the time I had finished my fifth year as a graduate student in the university, there came a slow broadening of the horizon; for each year my horizon had grown wider and wider, and the whole field more full of joy."

Facing contentedly the refusal of my colleagues to admit me into the local chapter of Sigma Xi for several years on the ground that psychology was not a science, and living to be the first of them to be admitted into the National Academy of Sciences as a representative of that science, is living on a rising scale. The progress of discovery and invention and the extension of learning have made the world over in kaleidoscopic fashion. We, ourselves, of this generation have been made over with it in our pretensions, in our opportunities, in our heritage of the age. To rise with such opportunities is to live on a rising scale, luck. And how can a virile American fail to do otherwise?

But not all those with equal opportunity count themselves lucky. Perhaps there is a reason for luck. I cannot refrain from quoting a passage from Irving Bacheller entitled, "Good Luck Picks Its Pals":

> "I have found that good luck is a wise chooser of its companions. It prefers to run with the man who sees his way clearly and is prepared to act quickly when approaching decisive moments. Luck is no laggard nor waster of time.
>
> "Then, too, it is always on the lookout for the fellow who has a conscience, and sense enough to obey it. This, however, does not wholly satisfy the fickle goddess. The young man must be eager to find his place and generous in the work it demands.
>
> "Every human being has a number of lucky breaks scattered through his life. Some people never recognize them. Others recognize them vaguely. The really lucky man is the one who knows good fortune when he sees it and then pushes it to the limit."

Heredity. I was born in Mörlunda, Sweden, January 28, 1866, which was also the birthplace of my mother, Emily Charlotta i Borg. Father, Carl Gustaf Sjöstrand, was born in Molilla, a village about five miles away. Father's surname had been taken by his grandfather in military service. Mother's surname is the name of the village, Borg, the *i* being equivalent to *in* or *von*.

The surname Seashore is a translation of Sjöstrand. The first of my uncles to come to America was at that time a young boy and went to a district school in Illinois. When he gave his name to the teacher, she found trouble both with the spelling and the pronunciation and finally in despair she said, "What does it mean?" "Seashore," he said. "Ah, Seashore," she wrote. He accepted it, and each of the branches of the family adopted it in turn as they came over.

The family history is interesting from the point of view of eugenics. The community from which both parents hailed was peculiar in three respects: the feature of being self-sustaining, the religious sanctions, and the eugenic form of the law of primogeniture.

The district is one of meager but diversified soil in which crop failure is practically unknown. It is divided into hemmans, a hemman being the amount and kind of land that would sustain a family, the family being estimated at ten or twelve souls. Climatic conditions being most healthful, the tastes and standards of living remaining fairly fixed for several generations, the returns from the soil being predictable with the assurance of "neither poverty nor riches," and the tradition that the family should live on "the fruits of the soil," being fixed, a remarkable form of budgeting prevailed not only from year to year but for family or generation, including plans for marriages and intermarriages with a view to reasonable sustenance. They lived off the soil, except for coffee and sugar; and these were used only sparingly.

All this had a most beneficial influence on the nerves of the people. They developed no nerves. The life, generation after generation, was so placid, so fore-planned, and so safe that this particular stock maintained a most remarkable record for physical vigor with mental and moral health. I took a walk with one of my grandmothers when she was ninety-three years old. All my grandparents lived an active life considerably beyond eighty. Although medical care was practically unknown, one saw few evidences of the ravages of disease. Childbirth was easy and safe. The people were inured to the cold so that, in spite of the severe winters, houses were heated sparingly only by open fireplaces, and churches and public places were not heated at all. They baked four times a year, butchered twice a year, and bathed when there was no ice in the lake. Wearing apparel was grown, spun, woven, and made up in the community group. By-products of the slaughtering found their way into candles, coats, gloves, shoes, saddles, and upholstering of the family carriage. Every drop of the blood was utilized. Hog bristles went into waxened tips. Coming events were predicted and prepared for. The distribution of inheritance, provision for old age, necessary sloughing off from overpopulation of the community, were thought of like the events of the day. Coffins were made in periods of leisure and were stored in the attic for the the old folks. Men took pride in the design and making of their own coffins. Marriage by dowry was the rule.

The wholesomeness of this stabilization of the means and the ways of sustenance can hardly be overestimated. It was simple living in all its beauty and effectiveness. To these conditions I owe the inheritance of a sound physique through more than a score of generations.

But back of this provision for sustenance came religion, the spiritual force that favored their adaptation to nature. Although the formal and more or less unspiritual state church held sway legally, my forefathers for many generations back belonged to a sect called Pietists. The above-described stabilization of life in this colony made the originally eruptive and emotional pietism, which swept Northern Europe, here most balanced and reasonable. The worshippers not only believed in God, but construed every individual and social act as a stewardship accountable only to God. Worship was as regular, as natural, and as enjoyable as eating. Temperance, civic virtues, sanctity of the home, and charity were God's laws, and therefore, the laws of His "children," as they fondly styled themselves. Here was a religion that functioned. It regulated the lives of the people and carried them to the end of the "journey" with peace in their souls. It was the simple life spiritually which really maintained and made possible their simple and natural life physically.

The genetic principle, which in a sense was the outgrowth or the expression of the above two principles, proved to be a biological tool favoring the law of primogeniture. Large families were the rule. My father was one of eight children; my mother one of nine. All these seventeen grew into manhood or womanhood and, without exception, each raised a family. This was typical in the community. It was before the Rooseveltian war on race suicide. The rule of the community was that only one of the family was to inherit the hemman. The rest must intermarry or migrate. Now, the rule provided that the eldest son should be given an equity in the estate and should carry on the traditions of the family, if he was worthy. Here is where the eugenics came in. For this purpose he must have proved himself acceptable by the family not only in his marriage prospects but also in the worthiness of his character. On these grounds the family really selected the inheritor, only giving fair hearing to the sons in order of seniority. This, it can readily be seen, proved a most effective leverage in the moulding of character, not only by selection, but also by the incentives furnished for the maintaining of the ideals of the family. It tended to preserve the conservative element in the group. Often the more enterprising and venturesome, as in the case of my father, chose to migrate.

For the frailties I have, I cannot blame my forebears. To them I bow deep in gratitude for the inheritance of a good stomach and a good brain, though I have much abused my cherished birthright. Let me give but one example of the biological change I have witnessed and in part have lived. My grandparents had good sound white teeth into their high old age, tooth brushes and dentists unknown. My mother had all sound teeth at the age of forty-three, at which time she was stricken with an illness which affected the gums of her teeth. I had sound teeth without a single cavity when I became of age. My four sons have all had extensive dental treatment from early childhood. Here is a biological change, all in four generations! Reflecting that the character of the teeth is an index to the physiological well-being, what a tragedy! What does it mean? What are the forces back of it? What are its accompaniments and its results? Are there any compensations for this transformation? Well, I would not go back to the original trail, yet I view with profound gratitude the lives of those who trod it and brought me into the world with its blessings. The change is a part of the world change. Even that home community has caught the fever—the craving for faster living. Would that I, as an educator interested in the conscious direction of the evolution of future society, could draw from such facts the lessons which may help to stem the tide of this rushing, overheating response to new opportunities by bringing future generations closer to Nature and closer to God.

Education on the Farm. There was no school in our community until I was eight years old, yet I had made good progress in learning by that time. My parents had taught me to read Swedish. Their first and only trick lay in using a primer which had a picture of a rooster at the back of the book. Every day I had done my lesson well, this magic rooster would lay a penny the following night. Reading had to be learned by aid of a pointer which my father had carved nicely for me. Laying the pointer with the rooster facilitated the laying of the penny. I can at this moment feel myself hanging in the balance between feelings of fact and fancy as to the mechanism and reality of the process.

But the laying season soon passed and I took more serious reading and rewards. In this my parents' main task lay in the answering of questions which my reading raised. By the age of ten or eleven I had read the entire family Bible, the Apocrypha included, and much of it several times. It is of psychological significance that, by the time I had reached the Apocrypha, the wisdom expressed in those books seemed to me to be superior both to the stories of the other parts of the Old Testament and the gospel of the New. Volleys of questions were raised by that reading, and, aided by sympathetic discussion, I acquired the art of reading in search of meaning. It became all the more interesting because it was hard to find enough time for the reading in my busy life on the farm. This I regard as a very fine incentive.

I had a remarkable memory as a boy, both logical and rote. After reading a chapter I could reproduce the content with great fidelity, and by two or three repetitions I fixed every detail verbally. I visualized everything in form and place on the page and was aided by both auditory and motor imagery, the last two being rather pronounced. The rote memory in reproducing faithfully what I did not understand, I think, was accounted for largely by my reading of meaning into it, however false and fanciful the meaning might be. This principle is well illustrated in our psychological demonstrations of forced associations in modern mnemonic systems. A wrong meaning is as good a memory aid as a true one, and often the more fanciful, the more effective. Thus what is regarded as rote memory may in reality be logical learning in such cases.

Memorizing was in vogue. I remember reading a Bible history three or four times and then reciting it chapter after chapter by memory, aided, of course, by the previous reading of the original. The content was appealing to me, and to "know" a passage was like digging up a gold nugget. The same method was followed in the study of history in the district school. The teacher with Venable's *History of the United States* open before her would say, "How far can you recite this morning?" Luther's catechism, both short and long, gave me good practice at home, and I soon found myself involved in theological discussions with Father. To me these discussions proved a way of finding God's will and Father's version of it, both infallible.

Before I was confirmed, I had the privilege of committing to memory verbatim three long and short catechisms. The first one was the one used in Sweden, the second one an American Lutheran catechism quite different, and the third was an English version of the second. A wonderful training in theology this was. It satisfied my craving for clearness and finality in such matters.

Although we had no public or parochial school during those early years, we did have a flourishing Sunday School in which Father was the moving spirit. Father led the singing and played the accordion until our family came into possession of a reed organ. What a treat it was for us youngsters to pour out our souls in song in the stately Lutheran chorals so full of harmony and chaste devotional expression.

Lively contests centered about the recitation of "verses" from the Bible. For me the competition element loomed up large; because, as later in the spelling school, I was usually able to maintain my position at the head of the class. I remember one Sunday when I submitted as my "verses" three chapters from the Gospel of St. John and the teacher said, "That is fine, Carl, but we do not have time to "hear" them this morning. You may hold your place at the head." This may have been interpreted as an exhibition of favoritism on account of my father's position as head of the Sunday School.

The teachers did not talk down to us. The whole program was serious and ennobling. Rotating the school from house to house was also of great social value. Of course we had no conflicting diversions, and were all tranquilly religious, craving this outlet. The profit and pleasure varied, of course, for different pupils. I fancy seeing the defense mechanisms of children not on display in music or theology.

Father, having some ambition as a public speaker, maintained an interest in "correct Swedish," which proved a valuable asset in our home. No slang, no jargon of any kind. He was a great lover of music. Singing was a part of our regular diet. Morning and evening worship was enriched by good singing. Singing good songs with a deep feeling of joy proved a good beginning in musical education. Our little reed organ was the first one within a radius of many miles.

Our district school house was built when I was eight. Father built it. He was the first Director. Up to that time our language had been exclusively Swedish in the entire large community. We knew no English. In his practical sense, Father arranged that the teacher should board at our house, not only for the comfort of the teacher, but in order that the English language might thus be thrust upon the entire family. This was kept up for years and was very helpful. We certainly kept that teacher in action morning, noon, and night.

The first day of school was uneventful except for me. Not knowing a word of English, never having seen a teacher, I started out with my dinner-pail, also a new experience, and with palpitation of heart I stealthily opened the door for a glimpse at the situation and my eyes fell upon the teacher, who rushed toward me. This pulled a trigger, and I shot off for home as the arrow flies. This was repeated on the second day. No other pupils came. After a sympathetic family confab, we decided that there were two girls in a neighboring family that should go to school; so, on the third day, I sought the company of these two girls and together we stormed the fortress. The teacher, overjoyed, kissed us all welcome and took the three of us together in her lap. That was an embarrassing moment. Luckily there were no men around to see it.

To the intellectual embarrassment social restrictions were added. Father had a law passed that only English could be spoken on the school grounds. So we jumped over the fence and played "one old cat" in the pasture and used our mother tongue in natural self-expression. It was hard enough to think in English; to play in English was asking too much.

All of us, knowing one language well, made good progress. I made the first three readers in five or six weeks, although the meaning of it all was in large part Greek to me. Gaining the ability to speak English furnished a thrill and proceeded rapidly.

Arithmetic furnished another revelation. I was fortunate in getting my start in mathematics under this teacher who did not know fractions; for I discovered the fact, that if you read all the rules carefully and work all the examples yourself, you will not encounter any difficulty. Think of it, starting in mathematics without lectures, without a teacher to work the difficult examples! The habit of confidence thus formed stood by me, so that even in college I worked ahead of the class in order to avoid interference with my mental operations. In my sophomore year Professor Uhler, spying me on the front seat day after day idling away time, once said, "Seashore, you are loafing." "Yes, sir," I replied. "Well," he continued, "we shall now take up conic sections. We have an excellent textbook. I will excuse you from class if you will prepare for a thorough examination on the whole subject." What a treat! Of all my teachers in college this professor is the most beloved. In a surprisingly short time I presented myself for examination and passed with a grade of 98½. Thus I am deeply indebted to two teachers, the one who did not know fractions, and this professor who realized that it is not necessary to teach mathematics to a student who wants to learn it. This incident forms the taproot to my later interest and insight into the Gifted Student Project, of which more later.

The demands upon my time for work at home were such that I could not go to school in the summer and we had only a three-month winter term. From the age of eight to the age of sixteen I probably attended the public school less than 600 days in all. On this slim foundation I entered the second year of the three-year academy on a fluke, the fluke being that they examined me mainly in mathematics, English, and history, and the examinations were based on the books I had studied. These books I knew. Of other subjects I knew practically nothing, as was also the case of other books on these subjects. My largest shortcoming was the lack of general reading of books.

The extra-curricular activities of country schools in those days were the spelling school, the singing school, and the Sunday School. The Sunday School continued to be a potent agency in character education. Through it Father, always at the head, carried over to us children of the pioneer community the heritage of the deep and tranquil religion of his own youth. He remained to the end of his life a confidant of the youth of the community in matters of religion and social adjustment.

Membership in the singing school was a result of natural selection. We were drilled in the sol-fa system and sang songs at our level. I sang soprano and had a remarkably fine, pure tone that was the envy of all the girl sopranos. We sang just for the love of it. There were no examinations to be passed, no choir to be maintained, no audience; only those who loved to sing came, though it must be confessed that there were other attractions to the singing school than mere singing.

The spelling school was also an institution for those who had survived in a sifting process. It was a convention that the local teacher must take her place in line and be spelled down. This was a stimulant in effort to some of us who participated in many districts. I early discovered a trick. The standard book from which the words were pronounced was McGuffy's *Speller*. All an ambitious fellow had to do was to master all the words in that book. Most of the boys of that day, as of this day, stared at the book as a whole and regarded it as impossible. But I discovered something simple enough. I went through the book, checking all the words I could not spell, studying each intensively as I went along. Then I went through this checked list in the same way. Soon the whole slate was clear and I could stand up against any teacher in Boone, Webster, or Greene County. The last word conquered was "phthisis." On the basis of this discovery and the feats to which it led, I claim priority in the discovery of some of the current methods of teaching.

These gatherings were also social events to which we "took girls," driving miles and miles on wintry nights with sleighs and bells. We learned more than spelling in those schools.

Life on the farm was the most vital part of my education in boyhood. I look with pity upon modern efforts to teach manual training and agriculture to children from the farm. We of those days had the real thing. It was Father's theory that the boys should learn early to have a hand in everything that was to be done on the farm. I early had a hand in the selection, breeding, feeding, and marketing of animals. There was building and painting, buying and selling, shopping, and helping the neighbors. In advance of my time I was interested in diversified farming and introduced a pound of alfalfa seed in 1882. Being the oldest in the family, I was naturally entrusted with much of the overhead work in all our movements. Much of our work was done in the spirit of play. Cattle and horses roamed over the prairies primeval. We had to ride and round them up. Yes, ride! There was the life of the cowboy. Stunt riding was the rule. The small boy must ride everything. A long-horned bucking ram gave me great sport, but this grew tame because one could hold onto the horns. There was a big, ranging boar with nothing to hold. Calves for a while furnished amusement, but they were too easy to tame. Riding the prairie long-horned steer bareback furnished the real test of ability. There came out spontaneously many of the stunts I have since seen performed on the range or in a rodeo by a cowboy. No audience was needed, because companionship was with the animals. I loved horses and some of them were very affectionate to me. As a boy I did not play with boys, I played more with horses. We had but few play traditions and I did not crave even those because so many of the things called work were play, real sport to me. In this great diversity in play-work lay great educational value. It developed skills, self-confidence, patience, endurance, habits of motor coordination, and health.

An oft overlooked aspect of farm life is the time to think. In an intelligent boy, ploughing and milking—indeed, routine work in general on the farm—is done more or less automatically. No one to disturb him, he thinks. Many a day I have followed the plough barefooted, all the time thinking, planning, inventing tunes, thinking out poems, solving world riddles, or at least making up riddles, which was a stunt on the contest level among us boys. Is it more profitable to think in the armchair than behind the plough? Besides, such musings in isolation meet the need for periods of isolation and reflection as urged for all peoples of the adolescent period. Such a life of contemplation on the farm is to a thinking boy like going up into the mountains in the adolescent ceremonies of many peoples.

The final touch on my farm education was put on by Father sending me to town to live at the home of our pastor for the purpose of learning music and manners. I shall ever look back with gratitude for the fine cultural influences I enjoyed by living in the refined home of Reverend and Mrs. C. A. Hemborg. Mrs. Hemborg gave me music lessons, and at the age of fourteen I was made organist in the local church.

These educative influences of which I have spoken are but a few of the countless experiences. Sometimes progress was made through avenues of escape from hardships and danger. Other times hopes were realized by discovery of new means of acquisition and achievement; and then again there were the sublime moments of life, both in the physical and spiritual nature in which one simply learned to stand humbly in awe.

Father was trained in cabinet-making and carpentry and built our house as he also built most of the surrounding houses for the next twenty years. This took him away from home a large part of the time and threw very large responsibilities for management upon me as the oldest in the family.

Among the first pests we had to contend with on the farm were the swarms of wild ducks, geese, swans, prairie chickens, and cranes which pounced down on the newly seeded ground and devoured the sprouting green. These birds came in flocks like clouds, and it was difficult to fight them because they were not afraid. Scarecrows they treated as familiar parts of the landscape. When one thinks of the conditions for hunting any of these birds at the present time, this pioneer manifestation of bird life seems almost unbelievable.

As in the country from which we had come, we began to live off the soil and there were very few of the necessities of life which we could not thus secure. Wheat was for flour, ground in the neighboring grist mill. Corn was for fodder. Milk, meat, and vegetables were abundant. Butter and eggs were a medium of exchange in the small country store.

Children were an asset on the farm. All work and no play, some would say. Yet much of the work of the farm was done in play attitude and this playing was elemental. I am thoroughly disgusted with Hamlin Garland's weeping about the hardships of pioneer life in Iowa. He and I were contemporaries in this pioneer life. He, with his artistic temperament, was out of his element. He has presented his pessimistic view to the world, and the reading world weeps with him in his suffering. I saw the same situations in a diametrically opposite light. Would that I could paint my picture with the consummate art that he has painted his. The vigorous pioneer and his children were in their element. We liked to do things and there were things to do. Resistance to the cold, struggle against the elements in every way was an opportunity for overcoming difficulty and feeling success. This kept our blood red and our muscles firm and our appetites good.

Under these simple conditions the association with neighbors was a thing which is difficult to understand under present social conditions. We had all faced danger together, we suffered hardships together cheerfully. The community was almost entirely self-sustained, depending upon its own resources. This led to all sorts of cooperation in the planting and harvesting of crops, in the care of the sick, in all community interests, and the neighborly hospitality was almost a form of communism. No one suffered so long as anyone was well provided for. The hospitality and mutual helpfulness of the pioneer can hardly be overestimated. It is one of the sweet things of pioneer life and one finds intense pleasure in living it over in memories.

It was in the spring of 1869 that my parents with my one-year-old sister, Emma, and myself came to this country. We were six weeks on the voyage across the Atlantic in steerage passage. During that time my sister and I were both nursed through the measles. We were fully recovered before landing. We spent the first six weeks in Rockford, Illinois, with relatives, and then came directly to Boone County, Iowa, where we settled and remained. Uncle Alfred Seashore met us at Boone with an ox team and drove us eighteen miles through prairie and timber, fording the Des Moines River. We immediately located on an eighty-acre farm plot a mile away from his house and built a white house like my uncle's, a mansion it seemed to us in that day. In visiting it a short time ago I found that it was not a third as large as I had remembered it, and the farm which I had paced across hundreds of times looked to me now like a garden patch. This characteristic change in magnitude from early memories had one exception; the lane from our home to Uncle Alfred's seemed to be a mile when I was sixty years as it had been when I was a boy. This distance has always been my standard of a mile and has therefore been revised and kept up to date as a constant standard of reference. But so far as I can tell, all other areas, distances, and other magnitudes are overestimated in boyhood memories. It is probably true that events, values, sufferings, achievements, struggles, and satisfactions of all kinds suffer from this distortion in retrospection.

In retrospect, then, my boyhood education was of a primitive sort, meager in formal book learning, but rich and powerful in the challenge to cope with big situations. The freedom for vegetating in out-of-door responsible activities of rich and varied interests was a valuable substitute for pressure in brain work in a formal school training and confinement. Ours was a prolonged kindergarten set in reality with necessity as a teacher. If children of today could be thrown into such a life, close to Nature, I should not worry about their later book learning. I am very sorry to see that the farmers today in Iowa do not realize the possibilities of farm life in the education of their children. They crave country schools patterned after the ward school in New York City. Even educators in Iowa are blind to the countless resources for education on the farm itself. Here is one of the greatest weaknesses of the consolidated school system.

Driving to town one day in the winter of 1883, my father said, "Carl, would you like to go to college?" That was an entrancing moment. I can today see the snow, feel the gliding of the sleigh, and hear the clear tones of the Swedish sleigh bells when the first possibility of that new world was opened for me. For I had a natural thirst for knowledge.

Academy and College Education. Within an area of fifty square miles around our home only one man had gone to college when Father suggested college to me. He had gone to Gustavus Adolphus College, St. Peter, Minnesota; so there I went. It had a three-year preparatory department and, as stated, I entered the second class. The President, Dr. Wahlstrom, at once took a kindly interest in me and throughout my six years in the institution continued as my most valued friend and guide. In a very real sense, he took the place of my father in our relationships. He stimulated love of learning and right living in the most wholesome way. In those days, right living was more of a goal in college education than it is today. It consisted in "preserving the faith" and doing God's will. The president was to us a sort of ecclesiastic intermediary to whom we were accountable in the first instance. But, strange as it may seem today, aside from minor regulations in the nature of information about proprieties which it was tradition to post on the inside of the closet door, it was not the standards or even the views of the President that we expected to conform to. Right in the sense of the will of God was the sanction admitted by the majority of the students. This was specially true of the academy students and the first two years of college. All the littleness, bigotry, censoriousness, and blissful ignorance bred in the small college of that day fall into insignificance in comparison with the character-building function of that sanction in our orthodox community.

This college is a denominational institution of the Swedish Lutheran people. Nearly all the students were of Swedish descent, a sound, sturdy stock, which has constituted so many wholesome ingredients in the melting-pot of the North Central States. Besides being of the most virile stock, comparison not challenged, the students of that day were selected by the fact that college education was not in the vogue, and going to college was evidence of a craving for knowledge and some degree of initiative and aggressiveness. Statistical studies would show that a larger percentage of the students of that day have risen to responsible leadership in their various walks of life than has been found in later years. The government, the professions, and the big business of Minnesota and adjacent states have been in the hands of the Johnsons, the Petersons, and the Andersons, who came into influential position through these pioneer families via the small college and later, in large part, through the university. I am simply recording a fact, a phase of pioneer life which is gone with all its hardships and all its blessings.

There was but one course, the classical, and that was meager; but like the country school, it had its compensations and the training we got tended to prove profitable in whatever direction we might turn. Mathematics and Greek were my favorite subjects. Mathematics had almost the appeal of a sport to me. Each new phase of the subject was a challenge which invited attack. It was exact, it had a system, it rewarded logic and effort. It came easy to me; and the competitive factor was significant. Besides that, it was the best taught subject in the college, partly because of little demand for facilities and instructional material, but primarily because it was taught by Professor Uhler, our most beloved teacher, still at this date teaching and beloved after half a century of teaching.

Greek, for four years, had an appealing charm. It was taught by the President, our next best teacher; but there was a fascination in the subject itself. The mechanics of the Greek grammar enlisted almost the same appeals as mathematics. Snatches of Greek literature opened up a new world of appreciation for me. I groped eagerly for each new element of insight into past culture. It furnished a point of orientation for my imagination to which I tied up history and other studies in a vital way. I am strongly audile and motile, although dominantly a visualizer. The ring of Greek words and the kinaesthetic dynamics which came from rhythm, accent, and meter hold a charm for me to this day. The scanning of classical selections was real play. The development of the habit of seeking for roots of words carried this auditory kinaesthetic word-value into all my use of language and is now a fixed habit and a constant source of pleasure.

As a result of my having to make the first two years of Latin in one year in the academy under a "stick" of a teacher, Latin failed for me. No fault of the subject; but I did not profit half so much by six years of Latin as I did by four years of Greek. Because of this deficiency in Latin, I suffer frequently to this day in a nightmare, or habitual dream, in which I find myself teaching Latin; and the real point of the nightmare is that I find myself not knowing the elementary forms of grammar.

Swedish, the language I knew and loved, was taught about the way English is taught in the average high school today, dry and crackling dust. I really loved and reveled in the reading I could do. If I could only have been freed from the classroom to live with Tegner and Runneberg, poetry and fiction, the sagas and folklore in my room, I should have been fortunate; for these had not only the literary appeal, but also the moral, mental, and racial drive.

German came easy to me and I liked it in spite of the poor teacher. I really got my insight to Swedish grammar through mastery of the German grammar. The kinship of these two languages was illustrated through my first experience in Germany. I naturally thought in German, whereas in French I must always translate. Much of this effective kinship of language is not really language, but in the common elements of culture, history, and national concepts and environment of the people. At the present time I can think in three languages: English, the commonest medium; Swedish, from memories of boyhood days and early religion; and German, in relation to German life and the psychology and philosophy I acquired through the German language. In recent years an interesting lapse has illustrated this dominance several times, as when I have spoken or had an impulse to speak Swedish in establishing intimacies or revealing personal confidence to persons who know nothing of that language. Psychologically, that lapse is interesting not only because intimacies or revealing personal confidences are marks of youth; but also because youth's attitude instinctively expresses itself in the language of youth.

I took French in my graduate work, and, as usual among graduate students, the subject was slighted. I spoke Danish freely as a boy and I understand Norwegian; but these languages seem to function ideationally only in reference to the local color they represent.

It seems to me that in current discussions of the value of foreign languages, the main point at issue is often missed. It is valued in terms of the extent to which it is used overtly especially in graduate-school circles. While the classics and modern languages are tools of investigation and media of communication, the greatest rôle of these foreign languages in my experience lies in the enrichment of the language symbols of daily life through the constant injections of roots, derivations, equivalents in translations, cultural connotations, and reverberations of racial life—in short, in the enrichment of connotations in our words in daily use. Indeed, I have often asked, "In what kind of a world of representation does a person who knows no foreign language live?" After all, does not the "cultural value" of a foreign language lie in the fairly automatized reactions with cultural knowledge as a source rather than in the facts about culture? The graft, or as the modern horticulturist says, the budding has not taken until it flows through our own veins.

Equipment for the teaching of science was inadequate and I never had a fair chance for a start in physics, chemistry, or biology, although I took courses in all these subjects. Psychology was taught under the head of anthropology, with Ribbing as a textbook. This book I practically committed to memory. Although the professor was a giant intellect, he simply antedated psychology. History and philosophy were taught with a high school technique. There was no scholarly background or penetrating plunge into the subject. The courses in religion were as dry as tinder, and futile in spite of the pulsating religious character of the institution and our dominant religious interest. There was no room for inquiry; it was not one-tenth as interesting as the reading of the Bible or committing the catechism to memory. It had to be academic and there was no scholarly point of view. The contrast between this formal instruction and the spontaneous motivation of religious life upon the campus was striking. The latter was a splendid moral growth of something living; the former, the fruit of the seed that died in germination.

A survey course in astronomy in the senior year was of great value, not so much in the facts acquired, as rather in giving me orientation as to my place in the universe, enlarging my responsiveness to the universe both materially and spiritually.

Intercollegiate athletics, we did not have. The value and need of physical education was not recognized. Swedish gymnastics were taught as an art; indeed, so well taught that for a year or two I had the ambition to become a teacher of gymnastics. No art, as such, was taught or even thought. Music played an important rôle in student life, as distinguished from formal musical art. The Swedish nation is a singing nation, because the people are whole-souled and healthy-minded and the climate is favorable for the nightingale voices.

In this life of song in college, I had some degree of leadership and found in it my sweetest pleasures and most numerous thrills. With us, it became an intramural competitive sport. We sang for pleasure. At one time I had a part in four different quartets in addition to the glee club. We were invited to sing, expected to sing, and loved to sing on all sorts of occasions. Yet our chief pleasure came from self-expression among ourselves quite apart from audiences. One of my stunts in the tours of our college Glee Club was, however, to sing a baritone solo so as to draw tears from the maidens in the audience. "Weep not my pretty maid. Fire of eye, rose of cheek shall not die, etc." It was generally known in advance that this condolence, sung to a humming accompaniment of the Glee Club, would bring the expected result, tears of appreciation.

One member of my class was a musician and we had a compact to the effect that he should give me music lessons in return for my tutoring him in mathematics. I managed to give him the drill that enabled him to pass his examinations without understanding the process at all. By contrast he instilled in me a real love of music with but little skill in performance.

My activities in music also took a practical turn. Through it I largely paid my way through college. On Sundays I served as Organist and Director of the Choir in a church at Mankato, twelve miles from St. Peter. I went over on Saturday evenings and conducted a combined singing-school and chorus rehearsal. I had an organized choir of forty voices, which was maintained intact for three years. The remarkable feature about this chorus is the fact that I charged an admission fee to all the rehearsals. I am rather proud of that record because it shows that it is possible to teach youth to love to sing to the extent of being willing to pay for the privilege. Members of church choirs today, please take notice! This working on Sunday was one of the most valuable parts of my training while in college. While incidentally it contributed toward self-support, it was to me a wholesome development of self-expression for pleasure and worship. It was a way of rendering community service.

In the present mass education and "mere" education, this feature is often sacrificed. To me the very going to school is a sort of hopeless and helpless thing. Many students are washing dishes in the restaurant because they do not have the initiative to discover a job in which they could take pride and pleasure and gain valuable training and at the same time make it a social service.

The chapel talker is heard to say, "You may learn more from one another than from your professors." I learned most vital lessons from the few men I admired and who were a class ahead of me. Because they managed to seem far ahead of me from year to year, I associated with these men rather than men of my class. In this class were Magnuson, the philosopher; Carlson, the literary connoisseur; Stone, the man with the social outlook; Eckman, the cynic lawyer; Nelson, the poet; Lundgren, the theologian; and other notables. What I gathered from my studies had to be passed upon and rated by these specialists. In my process of digesting the intellectual palaver of the college, they always knew the answer.

The nearest we had to a fraternity was a group of ten men, including these named above, who went by the name of Dekadelphoi. As our organization has broken up, I can let out our jealously guarded secret which cemented us in fellowship. We had a table of our own in the dining-room and made it a conspicuous practice of fraternizing with distinguished guests and visitors on the campus, seating them at our table and giving them a jolly time which constantly filled those at the other tables with curiosity in regard to the cause of the hilarity. The secret was this: when any one of the brothers told a story simulating reality, it was the solemn duty of the other brothers to support it with collaborating evidence regardless of its absurdity. For this purpose we developed a repertoire of thrillers. It would be difficult to match that device as an incentive to rollicking table talk and hearty laughter. It sustained the parrying in flashes of wit.

My college life was uneventful socially, and yet full of wholesome and happy associations. The debating societies loomed up large. The music organizations were essentially of a social nature and purpose. The Dekadelphoi, at and away from the table, formed a marked social clique. Our small social parties, excursions, and ventures were somewhat of a home-like nature. I was regarded as somewhat of a ladies' man. My long and tightly curled blond hair seemed to be an attraction. In visiting the college twenty-five years later, Professor Uhler looked at my bald forehead and said, "Seashore, the Lord has stricken you for your vanity."

I worked hard and needed to; for in the Sophomore class I accepted the challenge from my classmate, Edbloom, to see who would stand at the head of the class. I beat him after a three-year, intensely heated contest by a fraction of 1% in grades; and was graduated in 1891 as valedictorian of the class. I had also held the position of valedictorian in the academy. I neglected physical exercise and came to the end of my senior year a first-class physical weakling, for which no apology could be found. Coming from the farm with a rugged constitution and a farmer's heart, I had overdone the sedentary life and brain work at the expense of maintenance of a good physique. Fortunately, no constitutional defect had come from it.

Such was the life of the pioneer denominational college a generation ago. It furnished but little incentive to higher scholarship beyond college. The numbers were small, indeed; there were only four men in my graduating class. The equipment was inadequate; our environment was radically different from that rapidly developing in the state university. We lacked facilities for the type of competition offered in larger institutions; but on the whole the small college sent us out with two distinct advantages: first, the self-confidence and habit of leadership; and, secondly, a fairly good mastery of the college subjects from a secondary point of view. I have often wondered what the advantages or disadvantages would have been to me, if I had spent the four college years in the university rather than in a small college. I am certain that the result would have been different; but whether better or worse, I am not in a position to say.

Education at Yale. In the Dekadelphoi group, Magnuson, the philosopher, had stirred up in Carlson and myself an interest in philosophy. Carlson went to Yale to study philosophy; and so a year later, I followed him. I did not then know that Ladd was going to introduce me into psychology. Indeed, I had no clear notion as to what psychology might be; but I entered Yale the day the Psychological Laboratory was opened and on Ladd's advice entered the laboratory course. Four of us graduate students, Bliss, Buchner, Gilbert, and I registered in the Laboratory the first day. The other three men had each had a year or more with Ladd, Duncan, and Sneath. I carried Introduction to Psychology and a seminar in Schopenhauer with Ladd; Advanced Psychology, mainly James's two volumes, with Sneath; and Evolution, with Fischer. All of these courses proved fascinating because they were new to me and served a natural craving for this type of knowledge.

During my four years at Yale, I had a most delightful and wholesome home life, living in turn in the homes of friends, Reverend and Mrs. Enstam, Professor and Mrs. Andrén, and Mr. and Mrs. Hanson. These three families I can never repay for what they did for me. Home was no distraction from my studies and I had happy home comforts and associations.

There were about sixty graduate students at Yale that year. We were quite exclusive, close to the professors. We wore silk hats and Prince Albert coats to class. I had no association with undergraduate students except in Ladd's introductory course for three hundred students. No questions were asked in regard to my admission when Dean Phillips registered me. Ladd had vouched for me, on what grounds I do not know; but in those days that was law.

The course in the Laboratory was too rudimentary and too devoid of book learning to suit me. I resented Scripture's frequent reference to the futility of getting psychology from books, especially his speaking lightly of Ladd and Sneath, from whom I felt that I was getting things far more valuable than this laboratory stuff. I did not see then what laboratory experiments were about. We spent a long time on experiments which seemed nearer to telegraphy than psychology. Dissection of the brain then, as now, impressed me as only a remote interest to psychology.

Toward the end of the year, an incident happened which proved to be a sharp turning point. Although still busy absorbing Ladd's system, I began to feel the need of independent work on a problem; so one day I went to Professor Ladd and told him I would be interested in doing some work on the subject of inhibitions. "That is a pretty interesting subject," he replied, "you will find a full account of it in my large book, page 143 ff." We had not reached that point in the class, so I looked it up and, indeed, it was a very interesting account and for a while proved a solution for my embryo impulse for investigation. But the original idea that I could do something with that subject came back, and so I went across the street to Scripture and expressed the same desire. He listened intently and then slapped me on the shoulder, a very unusual thing for him to do, and said, "Try it." That "Try it," as opposed to reference to authority, gave me a fresh start, and from that moment I date my scientific birth as a psychologist.

Ladd's approach was still uppermost in my mind; but I became willing to try the experimental method. I failed in finding a good approach to the proposed subject; but I got my hands into the laboratory and soon found tangible problems of interest, although I was still untrained and did not have my bearings.

This incident is really representative of the struggle going on in the Department at that time. In America, Ladd's *Elements of Physiological Psychology* and James's *Principles of Psychology* were taken as the last word in experimental psychology in 1892. They both presented a masterly survey, digest, and interpretation of experimental research; yet neither one was an experimenter at first-hand in psychology in any noteworthy way. While James's books are monumental in original observations, he got Münsterberg to do the experimental work in the Laboratory. Ladd got Scripture for the same purpose and took a somewhat reserved and benevolent attitude toward him. I have heard Harvard men speak of James's experiment, as if he had performed only one. So far as I know Ladd performed but one experiment, and that by proxy. I performed the experiment for him. The hypothesis was wrong, the technique inadequate, and the conclusion was unwarranted.

In spite of my affiliations "across the street" (Ladd's reference to the Laboratory on Elm Street), I continued loyal to Ladd and carried a seminar with him for the next four years with great profit. I must have met with reasonable favor in his eyes, for in later years he selected me as the one he wished to entrust to revise his books as needed. He arranged that a financial proposition was made by the Scribners. Although deeply appreciative of the honor to a pupil involved, I felt that his works were of such nature that they should not be revised and did not accept the repeated proposal. However, I now recognize the one exception, the revision of the *Physiological Psychology,* which Woodworth later rendered so successfully. James's *Principles* will never be revised. No one has even had the temerity to revise the *Briefer Course,* although serious work has been done with that in view.

In the Laboratory I skirmished extensively before settling down to a problem for research. My first article to be published was a report on the measurement of speed of adaptations in accommodation of the eye for far and near points. So far as I know, the validity of those data has never been questioned. My next problem was the measurement of hallucinations and illusions. I produced hallucinations in the various senses by objective suggestion and compared the intensity of these with the intensity of the true sensation. Out of these grew the topic for my dissertation which dealt primarily with normal illusions of weight. Although the term may have been used before, that investigation gave status to and new interpretations of the term "normal" illusions. Up to that time the theory had prevailed that a person who was subject to such gross illusions was abnormal or at least a weakling.

A supercilious element in my attitude, as well as the attitude of my associates, was illustrated by the fact that I selected divinity students as my subjects for these experiments, it being generally conceded that these gentlemen, as a class, might be regarded as suggestible; but I produced a rather telling shock in reaction to this by turning the guns on professors and brilliant graduate students, showing that the normal illusion obtained for them quite in the same manner and degree.

As intimated, much as I now see the shortcomings of Ladd's system, I still maintain that he was a great teacher. His "system" rang like a bell. In this respect I have not known his equal, unless it be Paulsen, in Berlin. He had his subject thought out. His lectures were clear, convincing, and fair. He was a man of great erudition and had digested it all into his own system. He had an extraordinary power for balancing evidences and organizing logical arrangements of facts. His submission of fundamental issues to a rigid critique is often lost sight of because not stated in controversial form, but in his constructive and tempered style of review. In my own case, entering philosophy and psychology from such a constructive point of view was wholesome and pedagogically good from the point of view of natural growth. Having one self-consistent view in mind, it was easy for me later to entertain and evaluate conflicting notions.

Ladd had a profound confidence in himself, his philosophy and his mission. This "Lording" attitude he probably carried over from his earlier training and experience in the church both as a preacher and as a theological writer. It proved his downfall. He tried to impose his philosophy as science of sciences upon the sciences, history and religion. He regarded himself as Chaplain of the University and as spiritual advisor of the students, not realizing how few he reached. He had many supporters in the Faculty, but he overreached in his ambition to dominate. However, these weaknesses were very much like the weaknesses in men opposing him. The struggle grew into a pitched battle in which neither side got justice. The smash-up of the Department was an internal war, in which he and his supporters and associates lost. There was no compromise and many of the rights and merits in the Department were overlooked. Innocent parties suffered and the Department was cleaned out.

It took philosophy and psychology a long time to rehabilitate themselves at Yale. Scripture was swept out with Ladd, although they had practically nothing in common. Indeed, Ladd had never given a hearty support to Scripture, certainly no free hand. This cataclysmic fall of the Ladd stronghold had an unfortunate effect upon his influence with students and philosophies. His works should stand as classics in American philosophy, but his influence was broken by the loss of his graduate student constituency and the administrative collapse. Everybody was discouraged.

One of the unfortunate results of this as affecting students of the years immediately preceding was that this discouragement led them into other fields, so that, although during the decade preceding this collapse Ladd probably had one of the largest constituencies of students in philosophy, very few of them went on with philosophy as a career. The loss of the Yale backing led them to divert their energies into other channels.

The President should have recognized in Scripture the new approach to mental science, of which Scripture was champion. Instead he threw the baby out with the bath. Scripture came to Yale with good training and a splendid grasp of the approaches to the "new psychology." He was temperamental and enthusiastic. With the students he was both patient and critical. If he had enjoyed the freedom that came to Cattell and Münsterberg and Titchener, he would have played a notable rôle in the founding of American psychology as these men did. But the hampering under Ladd's administration and the final crash cruelly and unjustly left him stranded so far as psychology was concerned. All his pupils felt the same way and turned to other fields of learning for their life-work as he himself did. I am the only one of his pupils in that earliest period who survived the ordeal and remained clearly in the field of experimental psychology. There were men in that early group who were quite capable of a notable career if they had continued in psychology. Let so much of reminiscence of the downfall of psychology at Yale be enough of the sad story. I have noted the event here because the crisis was of national and international interest in relation to the sponsoring of psychology by philosophy in its first emergence upon the arena of academic respectability.

In the university as in the college I found unusual opportunity for self-expression in outside activities. Many of the students in my college were preparing for the ministry. While in sympathy with the profession, I had no desire to follow it and made no professions as a religious layman. But I had many friends in the Swedish Lutheran Church, which was rising rapidly in New England in that period as a result of the immigration from the North countries. I was strongly urged to render student assistance in the church as a preacher. This I refused to do at first, but I did consent to "lecture" on Sunday evenings by invitation. In this I developed some reputation by my ability to present in simple language and thought to the common people the ideas which I had gained in philosophy and psychology during the week.

I spoke readily either in English or in Swedish, mostly in Swedish, and usually took as my theme some practical interpretation or message from the philosophy or psychology I had studied during the week. I soon developed the practice of taking a Bible text, and thus by a gradual transition found myself actually preaching. The reaction of the audience was characterized by this remark of a strongly intelligent lady after the service: "When Pastor announces his text, we know what he is going to say; but when you announce your topic, we can only expect surprise." I may say that the surprise came not only to the audiences, but to myself from week to week in the preparation of these talks. I was bringing them something new, but in the meantime I was doing something very important for myself. I was trying to translate philosophy and psychology into its meanings in everyday life, and in so doing I built myself a bridge over from a narrow, orthodox view of my college days into a vigorous philosophical view of the world and of religion in particular. Not only did it carry me through that transition period, but it developed in me most valuable habits of leadership and thought, and fearlessness in dealing with new and difficult situations. These Sunday exercises were work of far greater training value to me than any course that I was taking in the University.

The pay was generous, but the real remuneration to me lay, first, in the challenge to integrate my academic learning into the real life of common people and, secondly, in the feeling that I was rendering a social service on Sunday. I never allowed these engagements to interfere with my week's work in the University. I did this work in the same spirit that a business man steps in and teaches a Sunday-School class. It therefore carried with it social advantages and the consciousness of apprenticeship in social service.

There were two events in Ladd's seminar that were pivotal in my readjustment. Each student had to present one topic during the year. The first year I undertook to defend Wallace's point of view in opposition to Darwin's, namely, that while evolution holds sway in all organic life, it does not apply to mental life. The conclusion stated as a result of my year's work was that Wallace was wrong and Darwin was right, just the opposite of what I had started to prove. Ladd was much interested in the argument, because on this subject he had remained evasive, often using the word "development" instead of the word "evolution."

The next year I undertook to ferret out what science has to say on immortality, starting again with a preconceived notion that a rather ponderable array of such evidence might be found. Again my conclusion after a year's work was negative. Science as such has nothing to say, either for or against the doctrine of immortality as usually expressed in religious beliefs. That was a moot question in the Department at Yale at that time.

In May, 1895, I turned in my thesis to Ladd, not to Scripture, although the work was all done under him and had his approval.

In his benevolent autocracy Ladd asked me what I was going to do next. I replied that I was going abroad as soon as my examinations were over. "Oh, we won't bother with any examination. You sail as soon as you are ready." So I sailed the next Saturday, knowing that Ladd would look after my interests, although I had made no application or formal arrangements for the degree with the Dean of the Graduate Faculty. Those were the palmy days of Ladd.

At the same time I was appointed Fellow in Psychology, which implied assistantship to Scripture. After the summer abroad I returned to the Laboratory and had two profitable years as a post-doctorate student and laboratory assistant. That was an unusual step for an American student to take at that time. These two years were largely responsible for such leadership as I have since enjoyed in psychology. At the end of my fifth year at Yale, I was appointed Lecturer in Education in Yale University, a new branch under the philosophical protectorate; but instead of staying for that, I accepted a position as Assistant Professor of Philosophy in the University of Iowa.

There was an incident at that time which did not worry me much, although it was one of the most fundamental determinants of my future. During my graduate work at Yale I had given no serious thought to the seeking of a position or even of specific preparation for a position. I had the good fortune of feeling free simply to do the things I loved to do as a graduate student. But as I was leaving Yale, I had an opportunity to go into educational work in China as educational missionary. This opportunity appealed to me very much, and there was a time when it was six to half a dozen as to whether I should go to Iowa or to China. I have since then often wondered what would have been my own personal development and my educational influence if I had cast my lot across the seas.

II. Formative Influences

Of the molding influences of educational and scientific character, enough has been said, but a word about some more intimate personal influences and relations may be in place. In my more mature years my association with my colleagues in this University has been intimate and rather extensive. For many years I have kept up membership in local university clubs in the various arts, sciences, and historical divisions. One dinner club, made up of nine members chosen from as divergent interests as possible, had its two hundredth dinner at my house a year ago. Of membership in learned societies, clubs, and interesting enterprises of national and international character I have had my full share and have been very appreciative of the privileges. Some honors have come my way. Among these I count the testimonial dinner given by the University in recognition of my thirty years of service. Another complimentary dinner given by my former students, together with the presentation of a portrait and a commemorative volume, gave me great pleasure. In my more mature years my association with the members of the National Research Council and of the National Academy has been a potent formative influence and has done much to chasten my points of view and stabilize my thought. My association with men representing various foundations, interests in research, and higher education has also been notable. It would have been a pleasure to write this autobiography purely from the point of view of "men I have known" which always makes vital autobiography.

As the earliest report of my activity is that I was "looking for mischief," many of my contemporaries will say, "You are still at it" because I have always been on the front battle line; and, on account of my great diversity of interests, have often been charged with interfering with other people's own business. In medicine, engineering, music, art, and war, in addition to education, I have broken in with my applied psychology and have fought for recognition of psychological service within all these fields. The formative influence of such varied interests has been very valuable. It should be said that this great diversity of activities does not represent as much scattering as it might seem to imply. The fact is that I have had first-hand knowledge on certain limited subjects in psychology and have reached out into these very divergent professional interests, contributing relatively the same element of applied psychology to them all. This has led me to the conclusion that to specialize very intensively invariably means an extraordinary spread of interests.

III. Family

I was born on January 28, 1866, in the village of Mörlunda, Sweden. My parents were holders of a "hemman," that is, a small farm, but Father also had acquired the carpenter's trade. This double interest he continued throughout his life; and, to these interests, he added that of being a lay preacher. Attention to the farm was, therefore, always of minor interest to him, as he was kept busy during the week building houses for the neighbors and often on Sunday conducting religious services. His activities are illustrated by the fact that during the last ten years of his active life he organized a congregation, built its church with his own hands, and became its preacher, deeply beloved to the end of his life. In the meantime he also continued to be the religious adviser and confidant of large numbers of young people in our own church. This absence of my father threw great responsibilities on me as the oldest son for doing everything that needed to be done on the farm, including the management of the hired help and care for the family from very early years. This was an excellent school and I can imagine no more effective training for the development of personality and ability to meet new situations with confidence and carry responsibility for continuity of undertakings.

My mother was two years younger than my father, having been married at the age of eighteen. She was in excellent health at the age of forty-two, a wonderful home-maker, beloved not only by her own family but recognized as a mainstay in the community. The outstanding memories of her center about the sociability and hospitality of the home. Of the families in the community, we had the largest acquaintance, and, particularly on Sunday, the house was open and guests came and went, so that the Seashore entertainment was regarded as an institution in the pioneer community and was often looked upon by some people as a form of extravagance.

Father died at the age of 55, possibly from appendicitis. Mother died at the age of 63, probably from some intestinal disturbance. We were five children, I being the oldest. Emma married a merchant in a small town and they have raised a very remarkable family of seven children, all of whom have been educated and have developed unusual personalities of leadership and devotion to the service of society. Emma's outstanding characteristic is one of calm and serene sweetness of character. When we were children it used to irritate me greatly that, when I was mean to her, she would always smile sweetly without the slightest reproof. My sister, Selma, died at the age of 23, I have sometimes thought, from success. She was a very bright girl and, in addition to her intellectual interests, threw her energies into community activities to such an extent that her health broke down, although the report was that she died of pneumonia. August is the business man of the family, and he has carried this ability into church and education as the head of a junior college. He also has raised a family of seven children, all of fine character and noble promise. David went into medicine and is now one of the leading physicians in Duluth. As a boy he was a very lovable character, a general idol of the community, and this character trait has been carried into his profession and his social life so that people recognize in him a beloved citizen and servant of the community. I also have an adopted brother, Theodore, a cousin who became an orphan at the age of eight. He also went into the ministry and is now pastor of a church in Fresno, California.

These family items are perhaps worth mentioning to indicate the favorable and strong family inheritance and presence of traits of sociability, industry, and natural leadership.

In 1900 Mary Roberta Holmes and I were married in Springfield, Massachusetts. Our oldest son, Robert, was born two years after our marriage and the other three came at intervals of two and one half years, making a fine series for psychological studies. Robert did not go to school and had not been taught to read until he was in his eighth year. During the first two or three years, he went to school only half of the day, but this does not seem to have handicapped him, as he had the doctorate conferred upon him when he was twenty-three. His graduate work took an interesting turn. As an undergraduate he had majored in geology and he took his master's degree in the same subject preparing for paleontology. Then he got interested in psychology and majored within my own field of investigation, so that, in addition to being his father, I was head of his department, director of his research, dean of his college, and acting president when the doctor's degree was conferred upon him. He had two years of training in research as a National Research Council Fellow in Stanford University and is now Associate Professor and Director of the Psychological Laboratory in the University of Oregon. He holds the distinction of being the first son of a member of the American Psychological Association to be taken into that Association.

Our second son, Carl Gustaf, named after his grandfather, is a business man, district salesman for the White Motor Company. While, like our other three boys, he entered the university within the highest ten per cent on the qualifying examination, his *A.Q.* did not correspond to his *I.Q.* As he himself diagnosed it, he had "mental indigestion." However, he earned his degree in civil engineering and for his present job he had three distinct years of apprenticeship, two of them during his undergraduate years. One was in road construction, the handling of men; the other was a year in the Bureau of Standards where he learned the optical glass industry; and finally a year of apprenticeship with the White Motor Company in Cleveland, where again he began with the wheelbarrow and ended in the personnel division. I mention this in testimony of my theory that in business, apprenticeships of this kind serve very much the same purpose as graduate work in learned careers, and to testify to the fact that this type of education for business pays.

Marion Dubois was the artist in the family. He had an aesthetic nature with a high order of musical capacity and love for music. After he had completed his pre-medical course, he expressed the desire to go to Harvard and pursue for a while subjects which should be as far from medicine as possible for the purpose of broadening his interests. He elected music, art, psychology, and literature. During the summer following his junior year he was in charge of a boys' camp service in Maine, being a trained life-saver, and with three companions he was caught in a storm and lost his life while saving the lives of two of his friends who were unable to swim. One of these was a physician who brought Marion home to us.

He made no distinctions between high and low. He was friendly to the scrubbing woman in his dormitory and he was fond of visiting at the home of President and Mrs. Lowell. As an illustration of his ready adjustment, I might say that when I visited him at Harvard the third day after he had arrived there for the first time, I asked him what he had done the day before and he said that he had been "showing visitors around the campus." Hustling to earn a little spending money, he got his admission to the ball games by carrying the drum. One day the man who played the drum failed to appear. Marion, who had never had the slightest experience in playing the drum, rose to the occasion, took his place and played the drum in the Harvard band. He said it went first rate except at one point when the rest of the band did not play with him. His entire life from infancy upward was full of initiative, optimism, and sociability. On his tombstone is the apposite legend, "He lived and died for others."

It is characteristic of Mrs. Seashore that when the telegram came reporting his death she turned to me and said, "Now, Carl, is the time to practice our philosophy." This she said with perfect calmness, which was maintained throughout our period of bereavement. The "philosophy" to which she referred was this: that she and I had a compact to the effect that, if either of us was to be called away before the other, the one remaining should repress expression of grief and should take the attitude of being thankful for the wonderful time we have had together. This proved to be a stabilizing philosophy and fitted well into this bereavement because, from the earliest childhood up to the moment of death, Marion was radiant with happiness, goodwill, and service to others. The sweetness that comes in a bereavement of that sort has in it an element of the sublime that cannot be described—it can only be experienced. The great mass of condolences which we received at this time fell into two classes; one, the customary religious assurances and expressions of sympathy; the other, a report of some incident—"I saw Marion do so and so"—with appreciation of his beautiful life. Needless to say, the comfort and cheer of the latter touched our hearts. The following words in his memory by Margaretta Ball Dickson are expressive of our sentiment at the time.

HE CANNOT DIE

Why do we mourn? A soul has been in bud
But blossomed suddenly in fullest flower;
Met life's full test; embraced its greatest hour—
Has showed that man is more than clay. The mud
Of Life's soiled fingers can not touch him now.
How many parents mourn a soul that dies
While bodies live—a tissue of weak lies.
Why do we mourn when Courage wreathed his brow!
All flesh must pass. The *when*, it matters less;
The *how* will count for all the years to be—
If soiled and shackled or rejoicing, free
As he who laid aside his mortal dress
To glad another home. He cannot die
Who meets Life's tests with vision true and high.

Our youngest son is Sigfrid. He shows most clearly the Scandinavian traits in his build and temperament, full of life and robust interests. He won honors at graduation and is now a graduate student at the University of Oregon, majoring in psychology under his brother, and has just completed his first psychological investigation.

The greatest obligation I have to acknowledge in this personal history is that I owe to my beloved wife. She is six years my junior. On her father's side she comes of the same family stock as Oliver Wendell Holmes and William Holmes, director of the Smithsonian, and through her mother, her ancestry through the Bodley and Dubois families is traced back to early Dutch and French sources. As an undergraduate she majored in the classics with Phi Beta Kappa recognition and as a graduate student, in philosophy. She has regarded our life together as a partnership, and this has been recognized by the students who, when presenting me with the Commemorative Volume, inscribed one copy to her to indicate that they recognized her share in achievement. This was a very happy and appropriate thought. So, as I look back upon my many blessings, she, my companion, is the best of all that has come my way and has been the most potent formative influence in my life.

IV. Pioneering in Psychology

My brief career in psychology spans the formative period in American psychology. There was a time when I had visited all the psychological laboratories in the world, was familiar with practically all existing psychological instruments and special methods, had kept reading of practically all psychological books and journals up to date, and had met practically all the then living psychologists of any consequence. Such a personal span of the period of development of our now large field of science seems incredible to the student of today. I mention it to emphasize the fact that psychology has sprung up essentially within the memory of some of us still active in the field, although it is already maintaining academic status with the older established sciences.

The psychologists of my generation have witnessed the first appearance of such concepts as clinical psychology, with all its inroads upon criminology, mental deficiencies, and delinquencies; applied psychology with all its offerings in psychotechnology; genetic psychology as an experimental science; psychology of the arts and music, laying new foundations for aesthetics; psychology of crafts and industries, now so prominent and often disgraced by psychological fakers; behaviorism, with all its variants and revolutionary issues; mental statistics, with all its brood of counting devices, legitimate in spite of the superficial gullibility with which it has been handled; mental tests with all its paper and pencil stuff dominating mass interests in psychology during the past decade; psychoanalysis, with all its cults and complexes; individual differences, with all its magic keys to the human constitution; welfare, with all its more or less specious control of individual and society; educational psychology, with all its imposed task of re-vamping educational processes and objectives; psychology of religion as an attempt at scientific approach and often substitute for religion; mental hygiene, with its concern for more than one-half of all human ailments. It is a strange thing that a man now living can look back upon a period before such powerful movements emerged and witness their full-fledged sway.

In brief, I look back upon the beginnings of psychology when psychology was "pure" and knocked feebly at the doors of Science for admission. Throughout a period of thirty-five years of the formative period of psychology, I have had the pleasure of sharing in many of the beginnings of large movements.

We are told that those who have been asked to contribute to this volume are regarded as pioneers in psychology. Perhaps I may best give an account of my pioneering by reviewing briefly a few of the movements in psychology with whose inception I have been identified in some respects. I must limit myself to some of the things in which I have had primary initiative and influence, especially in my own University. Such an account will set forth a point of view which may be of historical significance.

As I cast about for a permanent mental set or point of view which has determined my attitude and influence in these new movements, I find it in the reported instance in which Scripture said, "Try it." From that time on, laboratory experiment has been my goal: measure and count in an analyzed situation; vary one factor at a time, keeping all other factors constant; limit the conclusion to the one factor under control; where there is no experiment there is no science. In all my labor in pure psychology and in all my wide sweep of interests and efforts in the expansion of psychology into new fields of applied science, this has been my creed and objective—controlled experiment.

The Psychological Laboratory. The Psychological Laboratory in Iowa was founded by Professor G. T. W. Patrick in 1887. As was the rule in those days, psychology was fostered by philosophy. In 1895, Dr. C. B. Gilbert came in from Yale as Assistant Professor of Psychology and devoted his time to the Laboratory for two years. In 1897, I took his place and have continued in this Laboratory without interruption up to the present. For continuity of active service in the same capacity in one institution, this may, perhaps, be the American record in psychology.

Gilbert had staged a controversy with his chief and, when he heard that I was to take his place he wrote, "Seashore, you are going to hell." In this case, as in many others, hell proved to have been merely a matter of personal incompatibility. It was, however, real, as it drove Dr. Gilbert, who gave great promise of leadership in psychology, out of the field. Gilbert's scare was a bogey. It is now gratifying to record that my relations with Professor Patrick have been and are among the most highly cherished associations both personally, as a colleague, and academically, in our mutual interest and sympathies in regard to the relations between philosophy and psychology. His attitude toward me and toward the Department of Psychology has been extraordinarily cordial and helpful. This I attribute to his catholic spirit and scientific insight. For forty years he was regarded as one of the best teachers in the university, deeply beloved by his students and colleagues. From the first he gave me the fullest freedom and responsibility for psychology consistent with good administration; yet his wholesome influence has done much to mold the character of the department all these years.

During the first years in the Laboratory I had no assistants. I was my own stenographer, my own mechanician, and my own bottle-washer. This was at the beginning of psychology. I was thrown upon my own resources for the development of the subject and the building-up of the Laboratory. I wrote my own laboratory manual as the resources developed from year to year. This was kept up in mimeographed form like a card catalogue patterned largely after the Yale course. I designed new instruments and built first models with my own hands. For about five years I lived in the Laboratory fifteen to eighteen hours a day. That was a good discipline, a blissful situation, free from all administrative care. Although I have since then lived on a rising scale with all kinds of associates, assistants, and assistance, the habits formed in those early days keep my heart warm for laboratory routine to this day.

The laboratory course in psychology is an American institution. German and French traditions assumed that the student beginning to specialize in psychology had had his scientific orientation in some other science. He was introduced into psychology by being permitted to serve as a *Versuchsthier* for an older experimenter. While Sanford early planned an introductory course in experimental psychology, this was largely of informational nature rather than a training course. As intimated before, Scripture was the first to organize a formal training course in experimental psychology and, as his assistant, I had a hand in the construction of this course.

When I went to Iowa, my first teaching interest and objective lay in the development of the laboratory training course, a course not primarily informative but intensive in fundamental drill exercises. Titchener's four-volume *Experimental Psychology* is the highest embodiment of that principle. It is to the discredit of American psychology that these monumental exercises of Titchener were driven into innocuous desuetude by the paper and pencil ravages and extreme forms of objective psychology. In the present status of psychology, this type of laboratory training has been crowded to the wall. I remain old-fashioned and insist upon formal training of this type. Failure to maintain this requirement is accountable for much of the slush and trash in the output from American psychologists of today. For the laboratory was practically *spurloss versenkt* by the War. In its place tests became the vogue. Witness the published contributions to psychology for the last ten years.

Psychological laboratories have individuality due to the vastness of the field, the necessity of specialization, and the possibility of blazing new trails in pioneering. Let me attempt to mention the outstanding features of the Iowa Laboratory under my direction.

First, a visitor would be surprised at the small stock of standard instrumental equipment and the large collection of original instruments which constitute the equipment of the Laboratory. The dominant interest of a laboratory has always been in the opening up of new fields, which has meant the building of new equipment and development of new methods. We often take great pleasure in scrapping instruments because we never scrap one until we have devised one which is decidedly better. On acount of this character of equipment, many of the drill exercises in the laboratory course constitute work with new instruments which point to new vistas of investigation and kindle interest in research.

Another feature of the Laboratory is the extension of the central laboratory into new fields of psychology on cooperative terms. Notable in this respect are the conduct and housing of the Psychological Clinic in cooperation with the staff of the Psychiatric Hospital; the sharing of the machine-shop and mechanicians with the Department of Physics; the cooperation with the Department of Otology in the development of instruments and methods for the Psychotechnology of Otology; the overlapping of interests and activities between the Department of Psychology and the Child Welfare Research Station, devoted largely to the psychology of childhood and infancy; the extension of psychology of speech into a service program and research in collaboration with the Department of Speech;

the long-sustained program of cooperation with the School of Music in the applied psychology of music; and more recently the extension of similar services into the Department of Graphic and Plastic Arts and Physical Education. Most conspicuous of all is the cooperation with the Department of Education in the various aspects of psychology applied to education and educational personnel. This is in accordance with an administrative principle for which I have perhaps been more responsible than anyone else during the twenty years of my deanship, namely, that of having single central laboratories for each science with extension of services into all directions needed.

This policy involves the breaking-down of departmental fences and the traditional individualistic entrenchment of a professor in his department, and the development of a rich integration of all types of interests, especially in research. As a result of this policy the Professor of Anatomy in the Medical School gives a course for us in Anatomy of the Vocal Organs, the Department of Physiology offers a course in Physiological Psychology, the Department of Physics gives an especially designed course in Acoustics, the staff of the Psychopathic Hospital gives clinical courses in psychology. In all such cases the utilization of the best facilities in other departments is so much enrichment of our laboratory equipment.

There is a reciprocal side to this, namely, the varied services rendered by the laboratory to other departments. Thus the Psychological Laboratory has sponsored the development of audiometry in the Medical School through cooperative research, and has practically sponsored the scientific work in music, art, speech, and physical education, always restricting its approaches to the introduction of experimental methods through the utilization of laboratory equipment.

It so happens then that the interests of research centered in the Laboratory have as wide a scope as the various interests served in the types of cooperation available. There is perhaps not another laboratory in the country which can record so many aspects of cooperative efforts in research through the introduction of laboratory methods into new fields. The policy of sharing equipment, both give and take, with other departments has resulted in great economy. Back of all these factors is a dominant attitude of frugality and self-help. It may be that the adequacy of equipment and the variety of its sources reflect a type of resourcefulness on the part of the staff which is characteristic of research by a pioneer institution. At any rate, there is great pedagogical value in forcing the student to build simple models of apparatus with his own hands. This principle, as against the principle of turning to elaborate permanent equipment, represents a dominant policy of the Laboratory. It represents ideals of economy, self-help, and ingenuity as a policy. There are signs of the passing of these with the passing of pioneer days. One of the evil tendencies is the growing demand of students to have things done for them and the tendency to spend money in the place of using their heads and hands.

Investigation would probably show that I have invented or done the overhead work in the designing and construction of more new laboratory instruments than any other psychologist. I do not mention this as a boast or an indication of superior ability, but simply as an indication of the fact that, in hearty cooperation with my associates, I have been successful in meeting a large number of new situations and have succeeded in stimulating cooperation. I have been doing the Tom Sawyer stunt in a happy group of willing workers. Perhaps such achievement as we record is due in large part to continuity of attack upon certain units such as audiometry, phonophotography, and measurement of musical talents in particular. One research student has stood upon the shoulders of another. Indeed this continuity of projects is a marked feature in this Laboratory.

Since its foundation, the Laboratory has had a steady and phenomenal growth not only in the enlargement of the fundamental instruction and research in basic laboratory procedures but also in the development of new fields, some of which will be mentioned in turn. At the moment of writing, the Laboratory is scattered in six different buildings, our policy having been to accept a bit of space wherever available; but during the current year, we are moving into new quarters which will furnish perhaps the most generous allowance of space and accessories for a department of psychology anywhere. The various branches of applied psychology, such as clinical psychology, speech pathology, psychology of the child and child welfare, the psychology of art, and the psychology of music are assembled in one building around the central unit. There are about seventy-five rooms available for the graduate work in psychology, and some thirty-five additional for cooperating services.

We may point with some pride to the continuance of publications from the laboratory during and immediately following the war period when most of the laboratories in the country fell into the hands of paper psychologists; it is a rather notable fact that, of the ten doctorates in psychology last year, eight had employed some form of laboratory approach for their research projects leading to a dissertation and four of these are continuing this year in post-doctorate work in experimental psychology.

The same general tendency has prevailed in the Department for a number of years and, with a statistical study of the output in psychology during the past fifteen years, I think we can show that the man who has good laboratory grounding is most likely to become a productive scholar in dealing with fundamentals.

The Psychological Clinic and the Institute of Mental Health. I have witnessed the gradual appearance of the term clinical psychology. For many years inquiries came to me in regard to the availability of a psychiatrist to treat mental patients, and I had to say that there was no one in Iowa. Cases were, therefore, referred to the heads of hospitals for the insane who, except in one or two cases, were primarily business managers. No adequate provision was made for the examination, not to mention treatment, of mental defectives and criminals. Therefore, many of the problems in this field came to mere psychologists.

With the cooperation of Dr. R. L. Sylvester, the Iowa Psychological Clinic was established in 1908. It was organized mainly for research; but Dr. Sylvester's interests were largely in the field of service, and for that reason the Clinic took that turn during the eight years of his occupancy. In the meantime we were agitating for the establishment of a Psychopathic Hospital at the University. This hospital was established in 1915. Iowa was therefore one of the first states to take this step of providing a research center for mental diseases at the University. Dr. Samuel Orton became the first director of the hospital and a working arrangement was established whereby the Psychological Clinic should be regarded as an out-patient service in the Hospital under joint control of psychiatry and psychology, and that special emphasis should be placed upon research.

This cooperative arrangement represents a decided step in progress. It was recognized that the psychiatrist should be in charge of examinations and treatments. The function of psychology was twofold, first, to promote the training of psychologists for work in schools and service centers; and, secondly, to conduct research. The chief psychologist was regarded as psychologist to the Psychopathic Hospital and a psychometric service was developed. Staff appointments were made as joint appointments in the two fields. Arrangements were perfected for medical routine services to mental patients; and, since the Department of Psychology is responsible for all psychology taught in the College of Education and allied fields and the Clinic was organized for comprehensive state service, this arrangement perhaps represented the most comprehensive program attempted up to that time in this country for the development of clinical psychology.

The consummation of that organization represents the realization of one of my fondest hopes and efforts. I have observed closely the conflict between psychology and psychiatry and feel that this solution of the problem was most reasonable and progressive. The Hospital with sixty beds for patients under observation, an out-patient clinic for all types of cases coming to the Hospital Clinic (West Clinic), and another out-patient service in the new East Hall in Department of Psychology (East Clinic), mainly of an educational character, led to a very large extension of the clinical service into the field of educational social service.

Dr. Orton, a brilliant man in his field, did much to initiate research activities and plan for expansion of the clinic. Unfortuntely he suffered a nervous breakdown and became involved in a medical school controversy, in which his case was lost and he resigned his directorship. The whole thing was a tragedy. Fortunately for research interests, the work of Dr. Lee Travis and his associates was not only continued without interruption but has expanded without any disturbance as a result of changing headship. The new director, Dr. Woods, is sympathetic with the original program and his leadership in the Clinic is destined to a most wholesome development in the same general direction.

As illustrations of the policy of the Institute to concentrate its researches upon a small number of specific projects I may mention two which are now operating on a large scale, namely, speech pathology and reading disabilities. Speech pathology is conceived in the comprehensive sense to involve investigations into the nature of such disturbances as the aphasias, stuttering, and emotional blockings, and all the intermediate range of speech disturbances up to and including the numerous forms of so-called normal speech inadequacies. Much stress is being placed upon re-education for good speech. There is now a program operating in the University by which every entering freshman is analyzed at the opening of the year with reference to speech adequacies, and wherever a defect of any kind is found formal treatment is immediately inaugurated with the objective that at the end of the freshman year every student shall be capable of good and attractive speech, which is an exceedingly important element in personality. The same sort of treatment is carried as an experimental unit in our experimental schools, including high school, the grades, and the pre-school. It is our purpose to furnish the public school system a model procedure in speech education and re-education.

A parallel unit is the analysis and treatment of reading disabilities. We have found that many freshmen fail because they do not know how to read. One year we found four types of infantile reading disabilities among freshmen. Ability to read is now set as a goal of education. A thorough analysis is made of the university students and the lower grades down to the pre-school, determining technically the character of reading disabilities and following this with specific re-education in corrective work.

Emphasis is placed upon the early discovery of mental defects and the organization of specific treatment for each. This will be carried into various fields as the resources of the Institute expand. The principle is applied this year by inaugurating corrective treatment of the students in the School of Music based upon a careful experimental analysis of incapacities and inadequacies in performance. For example, a girl who flats in singing is immediately set into an intensive training series in which this defect which might follow her through life is eradicated in a few exercises of intensive drill with objective control, provided it does not rest upon innate incapacity.

Another unit which is being formulated is the development of a systematic psychiatric survey with corrective treatment in the hope of discovering inceptive maladjustments in young school children when they are most amenable to treatment.

Thus, the program of the Institute is a program of preventive medicine on the mental side joined to the continual treatment in the Psychiatric Hospital and the Psychological Clinic. The conventional psychiatric treatment of cases is, of course, continued and enlarged, and a conventional out-patient survey usually following within the scope of a psychological clinic is maintained.

I have mentioned these to show how the Institute conceives its responsibilities for diagnosis and re-education, beginning early and extending into corrective work in a number of factors which are not ordinarily regarded as the subject of education or treatment.

Through the cooperation of the Dean of the Medical School, Dr. Houghton, and the Director of the Psychopathic Hospital, Dr. Woods, with psychology and allied departments, the University has organized the Iowa Institute of Mental Hygiene, the purpose of which is scientific investigation, treatment, and prevention of mental disorders. It is organized as an institute within the Graduate College with the Director of the Psychopathic Hospital as Director ex-officio, and the Dean of the Graduate College as Chairman of the Board.

This, it is evident, is a new type of organization for research and service on the mental side. The principal features are: first, the very effective cooperation of the Medical School, Psychopathic Hospital, the Departments of Psychology, Education, and Speech in a comprehensive research program; secondly, the maintenance of the receiving clinic (the East Clinic) in the Psychological Laboratory which makes it unnecessary to send children or adults to the Psycopathic Hospital for preliminary examination, and results in vast increase in the number of cases that come up for treatment; and, thirdly, that handling of clinical cases is restricted largely to the number and kind needed for purposes of research, research being kept constantly in the foreground as compared with service.

The two services, the West Clinic in the Hospital, and the East in the Psychological Laboratory are under the same staff. The latter serves as a general receiving unit which refers mental patients to the West Clinic and conducts re-education or other forms of adjustments for the remainder. It is hoped that this Institute may be adequately endowed in the near future.

Thus, we have abandoned the notion that clinical work shall be under the direction of a mere psychologist and are placing the responsibility for all examination and treatment upon duly qualified psychiatrists. But still there remains abundant room for psychologists, primarily in the field of research, where they have decided advantage over the ordinary physician or psychiatrist by virtue of their training.

One of the problems we are grappling with is that of the training of men for just this kind of work, primarily psychiatrists and clinical psychologists. Arrangements have been made for cooperation with all interests of the Institute in the building-up of training facilities for work of this kind, quite regardless of the formal work for a medical degree.

I have traced this development from the inception of the Psychological Clinic, patterned largely after Witmer's clinic, up to the present Institute of Mental Hygiene in order to indicate the character of my interests and activities in the direction of the development and fostering of clinical psychology and a whole-hearted effort to bring into effective cooperation the two large forces, psychiatry and psychology.

Educational Psychology. In 1897 I was appointed Lecturer on Educational Psychology in Yale University. I knew practically nothing about the subject and in this respect I was in good company in this country; but I had some faith that some applications could come out of my knowledge of general psychology. This work, however, I did not undertake in Yale University because I soon after went to Iowa.

From the very first, many of my research problems have dealt with educational situations, and I have approached educational psychology consistently from the laboratory point of view. For two years, beginning in 1900, I carried an experimental class in the local high school, making measurements on individual differences of children. This was one of the first movements of the kind. Later this interest took a specific form, namely, the educational psychology of music.

I have never taught educational psychology, but my influence has perhaps been felt in two ways. There is first the rôle that I have carried in determining the character of the organization of the College of Education in the University of Iowa. In the second place, I have made a consistent effort to develop a system of applications of psychology to education not only in my own administrative activities as Dean and Head of the Department but also in those of general university organization.

One of the most significant applications of this principle is the arrangement in our University by which joint appointments in the various branches of educational psychology are held in psychology and education so that within a certain subject the same man teaches the psychological theory and the educational application, and the Department of Psychology is responsible for the development and maintenance of work in educational psychology. This has resulted in the cordial relation between the two departments and increased and sustained interest in fundamental research as distinguished from mere solving of immediate educational problems.

It is a notorious fact that in the absence of such cooperation there have been recent tendencies to subordinate psychology to education and thus make it a mere service department. A survey of the character of such subordination in American colleges and universities at the present time reveals the most discouraging situation. The situation is ruinous to both parties concerned. Psychology loses its status as an independent biological science with countless problems outside of education, and education loses because there is no fundamental research in psychology to contribute. The whole situation becomes superficial. Educators have not yet taken much cognizance of this because the situation is so common. This factor is again, in turn, a phase of a larger movement, namely, that of building within universities and colleges, teachers' colleges which are self-contained for a period of four years. Such separation of the College of Education from the College of "Arts and Sciences" is a separation of education from learning.

Although practically every member of the staff in our College of Education at the present time has come up through Teachers College of Columbia, which is the progenitor of this type of organization, the president, successive deans, and heads of departments have abandoned all efforts to carry that type of organization into Iowa. In view of the fact that I have been so long in service while the personnel in education has shifted rapidly, my influence for the unification of education with learning has won the day, and the situation in Iowa stands out as a distinctive example of a college of education that is integrated with the university. This is the outcome of a continuous struggle for more than twenty-five years, but now seems to be fairly established as a principle.

My largest influence as an educator is, perhaps, represented by my work on the Gifted Student Project, of which I shall speak later. As a teacher-psychologist, I have taken my responsibility seriously and the elementary courses, in addition to the Laboratory, have been treated as experimental units for the application of new principles of teaching, usually principles based upon psychology. Of this, also, there will be a few words later.

Child Welfare. History will perhaps give me some credit for having originated the idea which resulted in the establishment of the Iowa Child Welfare Research Station, which is the parent institution of a large number of this sort now organized in different parts of the world. We carried on a campaign for three years in the State of Iowa, largely through cooperation of women's clubs headed by Mrs. Isaac Lea Hillis, a woman of extraordinary queenly and effective personality in leadership. We used the argument that, since Iowa has improved its corn, its hogs, its horses, and its sheep very greatly through the scientific studies at the agricultural college, the State might well ask its university to see what it could do to improve the normal child or at least improve the conditions of the normal child.

For the purpose of promoting this interest I prepared and circulated extensively a prospectus showing what the station should do. It is of interest historically that, at that time, the term "child welfare" was not in use in the present technical sense. My own inclination was to use the term "euthenics," but common sense dictated that it would be better to use the simpler terms. I still think that the term euthenics as coordinate with eugenics should be brought into general use to denote this field of activity.

The development of this Station and its progeny has been phenomenal and has amply justified the notion that scientific research can be applied to the betterment of conditions of child life. Since in these reports I may from time to time "point with pride," I must say that it was very gratifying at a recent celebration in the Station to find that all the activities that I had originally upheld in my arm-chair draft of the Station have been put into effect, and that the original arm-chair program is still a sort of fundamental constitution for stations of this sort. Only in one aspect did I miss fire, that was in not favoring the organization of pre-schools. I favored the employment of a large number of workers dealing with children in their actual homes, but the pre-school notion has come in and has certainly justified itself.

For the wise development of the Station we are under great debt to the late Bird T. Baldwin who was at its head during the first eleven years, and to the wise leadership now in the hands of Professor George D. Stoddard. As Chairman of the Board of the Station, I have had to watch with eagle eye particularly, first, the tendency to employ too many assistants at the expense of overhead leadership in different divisions; secondly, the desire to undertake teaching on a large scale; and, thirdly, the tendency to go into the field of clinical psychology. I have attempted to maintain an effective line between the Station and the Psychological Clinic. If any credit is due I should like to have it primarily for developing the notion that one should apply science to the betterment of the normal child.

Psychology of Music. I drifted gradually into the field of psychology of music primarily for two reasons: first, my love of music and realization of great possibilities in an unworked field; and, secondly, the realization that the work I was doing in vision was unnecessarily fatiguing to the eye and lacked the possibility of the type of rigid measurement which seemed to be possible in hearing.

I was led to begin by an incident with a colleague in my dinner club. A colleague, whom we called Van, was continually boasting what a fine ear he had as a violinist. Being somewhat bored by this boast, I said one day, "Van, we'll find out how good your ear is." That night I stayed up the better part of the night to rig up a tuning-fork outfit for measuring his pitch discrimination. This measurement aroused a mass of problems, first, in the establishment of the factors involved in pitch discrimination and, secondly, in the extension of the same principle of measurement to the other attributes of hearing and from hearing to control and measurement of the same factors and the functioning of these in the higher mental processes.

In developing this field from our point of view, I have been more or less justly the butt of criticism from the musical profession, but here I have been helped with one general principle in my scientific career, which is, never to stop and count noses. When I have a new idea I am not in the least concerned about to what extent that idea immediately has a following, but I delve into the situation and my first concern is to sell the idea to myself. This being done, I am utterly callous to criticism based upon lack of knowledge in the situation. This character trait I think accounts for a considerable number of my best successes as the proponent of new movements and ideas both in pure science and applied. In this work, as in many other fields, I have often repeated the old saying, "I pray the Lord to protect me from my friends, I can face my enemies." In the field of diagnosing of musical talent, I have had a rather extraordinary following, but unfortunately much of it a gullible and non-critical type on the part of people who would take an isolated element in my procedure and handle it as if it covered the whole situation.

In handling this field I was very fortunate in striking a field of applied psychology in which measurement is feasible and quite as objective and reliable as the average biological measures. In the development of the work I note the following principles. First is recognition of the possibility of a complete classification of factors involved in musical talent by the combination of two bases, the four attributes of sound and the different psychophysic and mental levels at which they function. This furnishes a permanent and verifiable classification for the organization of experiment. Second is the recognition that everything that is conveyed from musician to listener as music is conveyed on a sound-wave and, therefore, the interpretation of the sound-wave in a satisfactory record furnishes a complete account of the actual playing or singing. Third is the principle that beauty in music consists in artistic deviation from the rigid; ugliness, of course, will come through inartistic deviations. This gives us a concrete and tangible approach to every medium for expressing beauty or ugliness in music. Fourth is a comprehensive classification of psychological factors as the basis for the organization of research. Fifth is the discovery that for each of the media of musical expression we can find a zero point, which in a sense makes our measurements absolute. Thus, all artistic forms of intonation consist in deviation from a fixed pitch. This pitch, spoken of as the true pitch, is the zero point, and the measurement of the form and degree of deviation is quantitative and entirely objective. Sixth is the principle that vocational guidance is largely of a negative character, that is, our primary aim is to find out whether there is any impediment in psychophysic, or any other, capacity and to determine whether that impediment is remediable. Seventh is the injection into musical education of the idea that training should center about the acquisition of a progressive series of skills, that the student should be fully conscious of the nature of this skill at the beginning of the training, and be equipped with exact and rigid mastery by the aid of instrumental technique, such as correcting flatting by singing before the tonoscope or adjusting exactness in performance of rhythm in practice with the rhythm meter.

In the development of this work it was fortunate that I was able to record six of the most fundamental measures on phonographic records which made them available at negligible cost, standardized the procedure, and facilitated group measurements. In the use of these records I have stressed the importance of regarding each measure as specific and as only a sample of a large number of factors which enter into a musical profile. Fortunately the six measures adopted have proved to be among the most important so that these, taken by themselves, furnish a fairly good index to musical capacity on the sensory side. Fortunately, also, this standardization of these measures and their ready availability has made them valuable tools for various types of scientific investigation both in pure and applied psychology.

I should perhaps claim credit for the fact that these measures have stood the fire of criticism for these many years due to the fact that each one was the result of very extended preliminary experimentation in the laboratory. The reason for the short-livedness of so many current mental tests lies in the fact that they are shot off on the spur of the moment.

One of the most fortunate opportunities for the development of this applied psychology of music has been the situation offered in the Eastman School of Music. Through the work of Dr. Hazel Stanton this institution carried on a seven-year program for the purpose of validating my six measures of musical talent as a means of selection for admission to the Eastman School. The recently published reports from that institution are extremely gratifying, showing statistically that the institution is justified in basing admission of students mainly upon this musical profile.

Speaking of this, Mr. George Eastman said, "You have saved us vast sums of money and undoubtedly you have prevented much human suffering by the introduction of this procedure. But that is all negative. Can you not inaugurate a positive procedure?" My answer was, of course, "Yes," and as a result experimental units are now operating in certain public school systems where a psychologist is employed for the sole purpose of discovering and motivating those children who are musically talented. This is a positive procedure and, in the spread of measurement which now prevails in the public school systems, it is destined to play a very large rôle in the future.

Then there is the remedial side with instruments now perfected for the measurement of motor capacity in each of the fundamental skills involved in music and the adaptation of these instruments as training instruments for the acquisition of these particular skills. We are opening an entirely new and scientific approach to the pedagogy of music. It is safe to say on the basis of laboratory experiments that a specific skill, such as control of intonation in a specific feature, can be acquired in a very small fraction of time when working with instrumental aids as compared with the time ordinarily required by conventional methods of teaching, and that the skill thus acquired transfers directly as a function in musical performance.

Surveying the whole situation in a paper before the International Congress of Psychology last year, I analyzed the situation by pointing out the following achievements made in the last quarter of a century in the psychology of music:

> Analysis and classification on a twofold basis: the physical attributes of the sound-wave, and the traits of the musical mind essential for response to these; a theoretical analysis verified and extended through experiment.
>
> The measurement of specific capacities and aptitudes: a profile of musical talents representing a large variety of specific measurements on cognitive, affective, and motor capacities and responses in relation to each of the attributes of musical sounds.
>
> The measurement of the character, extent, and progress of achievement under the influence of maturation and training in terms of specific skills in the various phases of musical appreciation and performance.
>
> Vocational, avocational, and educational guidance on the basis of talent profiles, involving validation of available measures of talent in relation to musical achievement, the selection of pupils for musical instruction, and the diagnosis of impediments encountered.
>
> Genetic studies on the inheritance of musical talent, the development of the musical mind in early childhood, anthropological studies of racial types, and the evolution of music in primitive peoples.
>
> The foundation of musical aesthetics based upon the experimental studies in each and every medium for the expression of beauty in music as determined by intensive laboratory experiments.
>
> Musical training: organization of the pedagogy of music on psychological foundations, and the introduction and standardization of instrumental aids for the facilitation of training in specific aspects of musical performances, such as artistic control of pitch, rhythm, and timbre.
>
> The outcome of all these should result in the structural material for a scientific psychology of music, based entirely upon experimental findings.

At the present time I am engaged in the organizing of a project for the establishment of an International Bureau of Standards for the Science of Music. This is, of course, a natural outgrowth from my studies in the psychology of music, which would perhaps hold a central position in such a laboratory, because to this science would fall the responsibility of determining measurements of musical capacity, the establishment of aesthetic principles, the development of scientific pedagogy in music, the organization of corrective work and re-education in music, and so forth; and the operation of all these services would of necessity call for the adaptation of measuring instruments to put the factors operating in music under active control for measurement.

In arguing for this type of organization I have pointed out that the world will always need musical schools well distributed territorially for the purpose of training musicians, but the world could well get along with one thoroughly equipped and generously maintained central laboratory for the determination of the scientific facts underlying music, because these factors once established become universal and need not be repeated. Such a laboratory would be not only a bureau of standards to which contributions from all parts of the world might be referred as a clearing house, but it should offer opportunities for research to a highly select group which could unravel a new world of possibilities somewhat on the order that the Bell Telephone Laboratories are opening up in the field of scientific approaches to hearing and sound production.

The objects of attack in such a program are surprisingly many. In addition to those mentioned, the scientific study of instrument construction can be one of the most fascinating problems. The king of instruments itself would undoubtedly respond to marked improvement under scientific approaches.

Another project on which I am now working is the tying-up of the interests of arts and science of music and speech with the professional recording of movietone films, both of primitive people and of artists. It has been interesting to find that the scientific interests may be served in conjunction with the professional news-gathering and film-production for entertainment by having on the staff psychologists and anthropologists capable of making a thorough analysis of the situation to be portrayed in order that the essential features may be thrown into the foreground and thus make the collection of the original material serve not only the purpose of entertainment, but also those of science and art and place these before the popular audiences. This is one of the most gorgeous opportunities that science has had within the field of anthropological collecting.

Psychology of Speech. It has been very gratifying to find through laboratory experiments that the scientific principles which underlie the psychology of speech are in large part the same as those underlying the psychology of music and that the instruments and techniques for research in speech are largely the same as those employed in the psychology of music. I have perhaps made two distinctive contributions which enable this University to offer the doctorate in speech with self-respect. The first is that a student who is to qualify in this field is not to be taught primarily by a "Professor of Speech," but, beyond the more or less elementary work on the artistic side, by specialists in the underlying fields; that is, he is to study anatomy with an anatomist, acoustics with a physicist, phonetics with a phonetician, psychology with a psychologist, and speech pathology with a psychiatrist. He may then conduct his research within any of these fields of scientific approach, the most frequent being that of psychology by the technique ordinarily employed in the psychology of music. For this reason, we have developed the plan of staff members with joint appointment in the Departments of Speech and Psychology.

In the second place we have been able to transfer to research in speech most of the technique developed in the psychology of music. For this reason we have been able to make progress in the psychology of speech many times as fast as the corresponding results were obtained in music.

A third feature consists in the organization of remedial work. The voice is an index to character and is also a salient element of character itself. From infancy upward the neglect of voice-training represents one of the largest gaps in our educational system. The program which I have assisted in evolving and promoting consists of the development of scientific aids to the early development of speech in the home and in the pre-school and kindergarten. It provides a service for the twelve grades in our high school and in the freshman class of the University. In this service, children and students are analyzed annually to determine the presence of speech impediments of any kind, and a remedial service is provided and enforced as outlined in the program for the Institute of Mental Hygiene. This service has enormous possibilities for the building of character through the development of effective and pleasing voice as well as by the removal of impediments. While I take some credit for having originated this movement, its success is due entirely to the very hearty following of the project in the Department of Speech.

There is a most tempting field for research in the psychology of speech, both normal and pathological, and the development of the instruments and techniques in this Department has given great impetus to investigation.

At the present time I am working on a project which has most fascinating promise, namely, that of utilizing the public entertainment through speaking films for the purpose of cultivating an interest in, and illustrating methods of, training children for good speech. It is a notorious fact that, although speech is an index to character and plays a very great rôle in personal adjustment, at the present time very little has been done in the way of systematic development of speech in the child. This educational neglect is almost unbelievable. Now, if we can build a plan which illustrates the beautiful speech of mother and child under a variety of conditions and make a standing project of this so that people will come to look for short films on the stage and become familiar with children or mothers and teachers who have beautiful speech, we have an opportunity for offsetting the training in ugliness in speech which the film now represents through rowdy characters and the possibility of giving impetus to a great educational movement resulting in increased attractiveness and efficiency of personality through the possession of good speech.

The Psychology of Art. In cooperation with Dr. Norman C. Meier, we have been able to extend some of the same principles of research and service found in music and speech into the field of graphic and plastic arts. This has taken two turns: first, the development of the *Meier-Seashore Test Manual* to be used as a dragnet to discover art talent primarily in the eighth grade of the public-school system; and, secondly, the organization of a program of research on the analysis of the development of art talent in pre-school children.

This *Manual* has introduced a radical departure into the test-making program for art in that it has introduced into this procedure three fundamental principles of measurements that are recognized in the Laboratory: first, that measurement should be made only in an analyzed situation in which one factor is identified and varied under control; secondly, that this factor shall be presented in its true setting in an actual picture of recognized quality so that there shall be no distortion by evaluation of the factor under control; and, thirdly, that the conclusion to be drawn shall be specific in terms of the factor under control.

The adoption of these principles immediately discards all the testing programs in which the factor on which the judgment is based is not identified, as for example when a child is asked to say which of two landscapes or other pictures he likes the better. Dr. Meier's development of the plan of altering one specific element in the picture at a time and placing the true and the altered picture for comparison, introduces a new and fundamental principle in the measurement of art features which may be extended indefinitely and adopted as a tool for the purpose of answering countless varieties of questions.

The development of the program of genetic studies in the Child Welfare Research Station is our next fundamental approach, and, though yet in its beginning, is full of promise.

Psychology of Physical Education. It is interesting that when we make a fundamental contribution in scientific approach to any subject, such as music or art, we are confronted with inviting possibilities for extension of these same principles into apparently remote fields. Such a one is the field of sport, athletics, or physical education. Largely through the efforts of Dr. Milton Metfessel, we have been able to make a beginning in the analysis of talent for sports and various athletic contests, a field which is quite unworked but fully legitimate and promising. The psychological approach is advantageous because we already have a technique for measuring action, and we have the photographic and other forms of instruments which may be applied to movement studies, the development of skills, and similar problems.

I foresee a great future in this type of study, for example, in the ancient and honorable game of golf. We now have the apparatus in which we may record the exact position of the club and the form of the player in 1/1000 of a second throughout the entire swing. Given such a technique and skill in psychophysical measurements it is clear that research is certain to lead to exact formulation of principles involved in a stroke and undoubtedly to suggestions for improvement of the action in the light of critical studies of best players.

Physiological Psychology. I consider it an advance in the organization of psychology that we have largely eliminated the pure physiology instruction from the elementary course and that the course in physiological psychology is given in the Physiological Laboratory by men who are well trained in both physiology and psychology but have primarily the physiological point of view.

Comparative Psychology. In like manner it has been my policy not to maintain an animal unit for psychology alone but to maintain working relations with the Department of Zoölogy which has an elaborate layout of provisions for care and use of animals. This may be illustrated in the case of researches in child welfare. From the point of view of genetic psychology those engaged in research have not only the pre-school children but a unit for the study of new-born infants in the Hospital and a similar unit for the study of controlled experiments upon animals in the Department of Zoölogy.

The recital of these modes of cooperation and organization may seem trivial, and yet, from the point of view of effective economy in the organization of research and from the point of view of the maintenance of cordial relations and cooperation within related fields, this type of program has enormous advantages.

Psychology in Otology. In the *Bell Telephone Record* for April, 1926, my picture occurs over the legend "The Original Audiometer Man." One of my first undertakings when I came to Iowa was to experiment in the building of audiometers. A study of the evolution of these audiometers would make interesting history, showing two trends of development: first, making the instrument put under control more and more factors involved in hearing; and, secondly, a gradual simplifying of mechanisms for this purpose. It was a very interesting experience to be a party to the development of this new field.

One significant step was what we may call a new branch of service in psychology, namely, that of psychology in otology. The first full-time appointment in this field was that of Dr. C. C. Bunch in the Iowa Medical School, now in The Johns Hopkins Medical School. It may be predicted that a psychologist in otology will be employed wherever the profession is practiced on a large scale.

Mental Tests. I have had a hand in this from the time of the psychological experiments conducted in the Iowa City High School in 1899 and on through different situations, and my influence, if there has been any, has been in the direction of carrying laboratory technique over into testing outside of the laboratory. One or two little incidents may be of historical interest. Many years ago I was passing the President's office when he said, "We have 273 applicants for admission to the Dental College and we have only 125 chairs. Have you any suggestion as to how we can handle this situation?"

In a manner which has become habitual in my cooperation with the President I said, "I can take care of that," and he said, "Thank you." That was all that was said at that time. The next morning the Dean of the Dental College called to know what the procedure was to be. I then realized that it was a serious situation in view of the fact that the University of Iowa has no right to exclude any properly certified high-school graduate who desires to be admitted. We immediately took the Thorndike Intelligence Test, which was then just out, and designed a number of specific tests sampling the type of skill involved in dental work and gave the examination in the spring in Sioux City, Des Moines, and Iowa City, and repeated it for late-comers in the fall in Iowa City. The result was that a large number of the applicants were scared away by the idea of having to take an examination, and a considerable number of those who took it and were advised of low standing accepted the warning and withdrew voluntarily without discussion. After this winnowing there were only 137 who claimed the right to be admitted and of these there were twelve that we had slated for discard. Eight of these yielded to advice in personal conference, and the remaining four were admitted into the College of Liberal Arts instead of the Dental College which then took students direct from high school. These all failed during the first semester and were dropped.

Apparently the administration was pleased with the way in which this situation was handled, for the President called me in and said "I will give you ten years in which to develop a qualifying examination on the basis of which we may eliminate candidates for lack of ability." This was about the first of August. I called attention to the fact that we could not get faculty action because the faculty was not in session and that if it were in session there was scarcely any likelihood of securing a favorable vote. In his characteristic way the President said, "This is an executive order to the Department of Psychology."

I accepted the order and went to work, this time using the entire Thorndike unit, which I think was a three-hour examination. During registration week everything was set up for action in the armory on the pattern of testing which I had seen in the army mental testing. The faculty was dumbfounded at this action, and many expressed deep indignation. I happened to be absent from the first faculty meeting, and at that time a veritable storm broke out. One or two of the old professors spoke with tears in their eyes in behalf of the suffering student. If I had not previously acquired some stability of status in the University, this would have proved my undoing. However, the Dean gave a happy turn to the event by suggesting that it would be well to postpone action until the second semester at which time Professor Seashore may report what degree of success he had in predicting grades for the first semester. This was granted grudgingly. When the time came, I spread a sheet across the whole front of the Faculty Room showing the performance of those whom the tests had placed in the highest 10 per cent and in the lowest 10 per cent, respectively. Taking only the extremes was a strategy which some of my friends did not see through at the time. But the fact that the lowest 10 per cent had practically been wiped off the map on the basis of performance in the first semester was a shocking revelation to the Faculty and the fact that there was a very great tendency for those in the highest 10 per cent to draw the good grades was convincing to such a degree that at that meeting the Faculty voted to adopt the plan of conducting mental tests upon the freshman class during the registration week as a permanent policy.

I mention this because it is perhaps typical of what happened in many institutions in the way of obstructions met in the introduction of freshman qualifying examinations. With assurance of success based on these experiences we proceeded to build our own examinations and injected two new features. One was the separation of aptitude tests from achievement tests, and the other was in the direction of making the examination specific by making it a battery of placement examinations in each of the subjects to which freshmen were admitted. Both of these features were at that time quite unheard of, but they have been amply justified by their continuance in the development of the testing program and their quite general adoption. These examinations were among the first to be called "placement" tests.

At this time I was made advisor to the Board of Investigation of Engineering Education, and at the first meeting they asked me what procedure psychology would have to offer for the improvement of the organization of incoming students. I suggested two: first, the adoption of this type of placement examination; and, secondly, the adoption of the plan of sectioning students on the basis of ability. Both were approved by the Board and through its influence were given a very satisfactory trial in a large number of the engineering colleges of the country. Indeed it was through this impetus that the best advances were made in the development of placement examinations. In all this work my own responsibility was limited to a sort of overhead work. Professor Giles M. Ruch and Professor Stoddard were primarily responsible for the development of these tests in the first instance, and Professor Stoddard has followed up the work to the present time. I think it is generally recognized that the Iowa Placement Examinations are ranking well among examinations of this kind at the present time. The two features we injected are but a step in the direction of making intelligence-test batteries out of a series of specific units which will identify and describe them. I predict that in the future the sequel to the Binet tests will be, not a scrambled mass, but a battery with specific units, the significance of which can be fairly known.

One aspect of this college qualifying examination is the adaptation of it for use in the high schools in order that the student may have the information about his fitness for college before he leaves home or burns his bridges behind him with the decision to go to college. This examination which has now been conducted for a period of eight years, yielding predictions for from 1500 to 2000 high school students a year, and follow-up work tracing the performance of these students in college has fully demonstrated the validity of the procedure and its great usefulness. The excellent work which has been done on this series of examinations has again been credited mainly to Professor Stoddard.

Teaching of Psychology. While my principal life-interest both in my own work and in my administrative responsibilities has been largely in the direction of research, I have throughout my career maintained a special interest in the teaching of elementary psychology. The first year I was at Iowa there was on my desk a little book with the legend on the cover: "Thou that teachest another, teachest thou thyself?" With that challenge in mind, I set to work to hold myself responsible for actually testing out in my own teaching whatever principle of educational psychology I might develop and attempt to teach to others. This has proved a serious undertaking but very gratifying in many ways.

One of the first features which came out of this was my development of the manual of experiments for the elementary course in which no laboratory apparatus was needed. The effective principle underlying this type of experiment was that a period should be devoted to an intensive study of one specific element, as in the first experiment, the after-images, in which the student gained first-hand knowledge of after-images and demonstrated to himself something like a dozen of the laws of after-images. This principle was opposed to the then and even now prevailing principle of assembling snatches of different experiments. While some of the experiments selected are not popular today, the principle has been generally recognized and the book has had a good market for these twenty years without any revision.

The second feature which grew out of this was the establishment of the class experiment. The class experiment, according to my definition, is one in which the instructor manipulates the instruments on the platform under such conditions that for a whole hour each individual student in a large class shall participate in the experiment as if he were the only person working with the instructor. My measures of musical talent were all originally experiments of this type and still serve this purpose. Of this type of experiment we developed enough so that, together with the experiments in the manual, we had nearly enough exercises to occupy one period a week throughout the year. It has been gratifying to see the advantages of these two methods of experiment and the extent to which they have vitalized the elementary work.

Another feature was the emphasis on the idea that lectures should be either class experiments or general introductions to new fields in a somewhat inspirational way and that the student should be held responsible for getting the systematic factual material from textbooks. My book, *Psychology in Daily Life,* is a collection of lectures of this type. Another fundamental step in the elementary course was the sectioning of the classes on the basis of ability. This practice, which originated one hundred years ago, had fallen out of use and my class was one of the first to revive and put it into effective operation under present educational conditions.

My experience in the elementary class in Iowa constituted the basis on which I later conducted a large campaign in which the advantages and disadvantages of the practice were very thoroughly aired, and it has been gratifying to see the extent to which this practice has spread into all the non-laboratory courses of instruction, both in the colleges and in the secondary schools.

But it was recognized that this method is a compromise in which one merely approximates grouping of equal abilities for achievement. My next step was to substitute for it the project method by which each individual student works for himself, under ideal library and experimental conditions, and no one is hampered in the least by the quality or rate of work of his associates. The result of this method may be illustrated in the fact that while we aim to maintain the same degree of achievement as a basis for passing and fail about the same percentage of students under the project method as we had failed under the old system, when our passing mark was 70, the high achievement of the students in the upper half of the class is so very much greater than by former methods that our passing mark on the new basis of distribution of assignments dropped from 70 to within the region of the 25th to the 30th percentile. The students who receive A in the course now must be above the 90th percentile and those who get B, above the 70th on an examination set for the span of the entire class. This spread of achievement is the most striking example of the result of giving the good student a fair chance.

Thus it will be seen that the elementary course has been a laboratory for educational psychology. In addition to the features mentioned, we have tried many others, some of which have been successful and others have failed, but on the whole the procedure in the class has been a pioneering in which we have taken bold steps but always on good psychological grounds. While at this present stage I have given up all other teaching, I still retain a considerable degree of responsibility for the elementary course in psychology, which I consider by far the most important teaching in the Department.

Psychology versus Brands of Psychology. I have never been identified with any school in psychology. Throughout my participation in the development of this science, I have always welcomed new approaches, given them careful consideration, and enriched my approaches and modified my way of thinking as seems vital in a new approach.

Trained in structuralism of the Wundtian type, I rapidly adjusted myself to functionalism and enriched my point of view by absorbing freely from all new movements. In this attitude I faced the French school of abnormal psychology, the English group of psychical researchers, Hall's school of child study, the group of animal experimenters, the Freudian and other forms of psychoanalysis, the statistical methods as applied particularly to mental tests, the various brands of behaviorism, paper-and-pencil psychologies of various sorts, the Gestalt psychology, and the recent neurological and philosophical approaches to the theory of psychology.

I owe a great deal to each and all of these and their sequels and variants but give allegiance to none. As a rigorous experimenter I continue to plod along with the feeling that this point of view, and not my particular brand of it, is the point of view of psychology. If what there is of system in my teaching deserves a name, it is the name eclectic.

Publications. I am making no effort to review my publications in psychology in this autobiography. They must speak for themselves. Most of them are technical reports upon laboratory experiments.

A fairly complete annotated bibliography containing more than one hundred titles was published in the *Memorial Volume* presented to me by my students in celebration of my thirty years of service in psychology (*Psychological Monographs,* 1928, No. 178).

The School of Religion. My religious interests have been so adjusted that I have always been some distance ahead of the times and the community in religious activities. In the University of Iowa this took the form of chairmanship of a committee which aimed to organize religious instruction for university students. We experimented with various methods and throughout a period of twenty years maintained a wholesome attitude of the university community toward religious education. During the last four or five years this took advanced form in the organization of the School of Religion conducted as an experiment under national authorities in which full-time professors, representing Protestants, Catholics, and Jews, conduct a department of religion with the privileges and responsibilities of other departments except that the financial support does not come from the State.

While at my own request I was relieved from serving as chairman of this final organization, I have been active and have carried responsibilities for integrating the work of this Department with the research and advanced teaching in the Graduate College. Thus from a very hidebound Lutheran as a child, I have gradually found myself ignoring denominational bounds and outworn creeds. For the last twenty-five years I have been Deacon of the Church, first in the Lutheran and during the last fifteen years in the Congregational Church, but I have never made any compromise with my freedom of religious thought.

If I should characterize my personal point of view as one which is still ahead of its time, it would be the placing of emphasis on religious life as the beautiful life—a combination of truth, goodness, and beauty in a beautiful life. I would contrast this with the present tendencies of the pulpit and pew of the day which is the religion of efficiency. Efficiency is a lower goal than integrated beauty in life.

V. Administrative Ventures

For the twenty-two years that I have served as Dean of the Graduate College, I have had to justify and defend myself before my psychological confreres for leaving the paths of pure research and going into administrative work. My primary defense has been that administration of this kind is in large part a form of applied psychology; that scientific training in psychology is perhaps the best preparation for this kind of a job. My other justification has been that, since my primary interests have been in research and in the extension of psychology to new fields, the official direction of the policies of the graduate organizations has enabled me to render a larger service for research than I should probably have rendered if I had limited myself to my personal work in the Psychological Laboratory. For example, one of the functions of my office is to hold consultations in regard to the choice of subjects for research and the improvement of methods, covering subjects in all fields of knowledge open for investigation in this University. In this manner, either for good or for bad, I have had the opportunity of influencing and guiding hundreds of students annually in their approach to scientific work and of conference and cooperation with departments in the recognition of points of view and facilitation for their work.

But perhaps back of this there is another justification, the underlying cause, namely, that I like it. This is probably because the training of a psychologist fits him for taking an interest in the guidance of persons and for recognizing relationships among vastly varied fields of investigation.

A deanship, however, is a hazardous occupation, and it is therefore some satisfaction to me that I hold the record among Deans in American Graduate Schools as the longest in service. This continuity during several presidencies of the institution has given me many opportunities for the fostering of continuity in policies which I have sponsored. As I look back upon my tenure, I attribute it in part to a certain capacity for initiative, courage to pursue firm convictions, and ability to meet men.

The four presidencies preceding the present one were involved in serious controversies, but in all cases I think I felt that I enjoyed the good will of the administration. As I look back upon the period I notice very great changes in administrative policy, but these are as a rule paralleled with the change that has taken place throughout the whole country in educational circles. For example, twenty-five years ago, we were at the peak of faculty enthusiasm for faculty control and elected committees in all administration. The growth of the institution and the changing order of practice in business, government, and all forms of efficient administrative work has led to more and more concentration upon the executive responsibility and the relieving of faculties and committees from boresome and ineffective work.

In visiting other universities I have found that there has been a radical change in the last twenty-five years from faculty control to control by the President, a sentiment which I have sometimes expressed in this way: "Show me ten institutions which are progressive and in a healthy condition, and I will show you ten presidents who deserve credit for this; and likewise, show me ten institutions which are stagnant or deteriorating and I will show you ten presidents who must take the blame." In other words, as in big business, we have been forced to concentrate responsibility and delegate power; and this same principle applies on a smaller scale in the administration of graduate work.

It is somewhat out of the ordinary that the Dean of the Graduate College should have a broader interest than that of his own college. During the last twelve years, the Dean of the College of Liberal Arts has been a man who was very much interested in research, and I have continued my interest in the theory and practice of the organization of undergraduate work. The result is that we have worked together in an unusually effective way; particularly, he has given me the opportunity of introducing into the Liberal Arts organization most of the ideas which I have developed through my rather extensive studies in higher education, and we two with the President have constituted an effective triumvirate, presenting a solid front.

One reason for the success I have enjoyed in this office, together with the ability to carry on research and many other projects, lies in my ability to delegate power, both to colleagues and to staff services. It has been my policy to reserve for my own work the personal conferences with the graduate students, both in regard to their research and common interests and personal problems. My real job as Dean is to have heart-to-heart talks with students and instructors. In this I have developed a psychological technique which enables me to see character traits, strains, and countless personal issues quickly and incisively. Students have often pronounced this ability as surprising, even uncanny. Through such contact I have been able to shape the future of many a student radically.

The Research Council. I had an active part in the organization and early development of the National Research Council and was made Chairman of the Division of Anthropology and Psychology in the third year. This was a formative period, the period during which the new building was constructed and the policies of the Council were gradually evolved. My own attitude was rather conservative. I advocated the slogan, "We are in no hurry, we have all time." This acted as a damper upon numerous efforts to give tone to the Council under the pressure of temporary and sporadic influences and gives some consolation for the feeling that during those first years the Council was not accomplishing as much as it might. An illustration of this tendency is found in the jocular existence of a "committee for the suppression of all committees."

It was a delightful year I had residing in Washington. I cannot even mention here the number of projects which we considered and in which I took some initiative, but there was one which assumed rather large proportions afterward. This was the Gifted Student Project. While enjoying some leisure in the arm-chair, I tried to draw up an outline of the changes that would take place in education if an educational psychologist should have free hands to organize in the light of such psychological facts as are now available. This led immediately to my agitation for a number of specific projects which seemed to me should be imminent. The first of these was the sectioning of students on the basis of ability, on which I read a paper in the Council in 1921.

During the War I had taken an active leadership in the efforts to discover gifted students in the graduating classes from the colleges for the purpose of giving them a rapid training for war service. This seemed so reasonable a procedure that we conceived the notion of conducting this search afterward in the interest of guiding men into training for a career in science. This was the beginning of what came to be known as the Gifted Student Project.

The Division of Educational Relations under the leadership of Professor Vernon Kellogg joined the Division of Anthropology and Psychology in sponsoring this project and secured funds for its conduct. It took the form of sending a representative of the Council to the leading educational institutions of the country with a message and a challenge to the institution in the interest of the gifted student. While I was in Washington, Professor George W. Stewart of the University of Iowa went out a a representative of the project and visited a number of universities. After that I continued the work for six years acting as representative of the Council and the

Division of Educational Relations in particular, as a visitor to the universities and colleges during such time as I could spare from my own work. During this period I visited about 140 universities and colleges, in this capacity, and perhaps a dozen or fifteen more indirectly in the interest of the gifted student. The visit came to be made an occasion for a sort of field day in the interest of good students. During a day of the visit there would be conferences with the President and other administrative officers, and usually a luncheon with discussion followed in the afternoon by a faculty meeting; in the evening an all-university dinner, which usually led to a two- or three-hour after-dinner discussion of the problems in hand. The effectiveness of the work depended largely upon the very systematic way in which the Division of Educational Relations made appointments and carried on the follow-up work through publicity.

It met with an extraordinarily hearty response because there was a general feeling that the good student was neglected. Speaking at Harvard, I said, "We ask only that the faculty should do as much for the encouragement of the good student as it does for the poor student. This is not being done." President Lowell spoke up with vim, saying, "That is true. Harvard never has done as well by the good student as by the poor student." And by that he meant that no institution had done it.

The work was facilitated by my ability to present the situation in high lights. For example, I would throw into the foreground the idea that it is the function of the teacher to keep each student busy at his natural level of achievement in order that he may be happy, useful, and good. It was then a natural thing to lead out and show how institutions do not do this and how the five-talented student as compared with the one-talented is the neglected and retarded student; this led to the presentation of various devices for the organization of the college. For this purpose I had collected information about a large number of current inceptive movements tending to serve our purpose and from these, in the spirit of post-war times, I made it a practice to keep a list of fourteen which came to be known as Seashore's Fourteen Points, being fourteen projects which I aimed to promote. A sample of these projects is the following list:

1) An advisory college qualifying examination, differentiating natural aptitude for college work from training, to be given at the end of the high-school course.

2) Departmental placement examinations for the orientation of the student within the department and for the furnishing of a general profile of student capacity to the administration.

3) Sectioning of classes on the basis of ability.

4) Organization of instruction on the basis of individual, group, or project methods, permitting free progress of the individual at his natural level for competition and progress.

5) Honors systems, including honors courses and other forms of free and competitive work for distinction.

6) Honor credit, or the gaining of time on the basis of superior work.

7) Elimination of competitive introductory courses and the organization of single basic courses in which different levels of progress are recognized.

8) Placing the ablest teacher in the department in charge of the basic course.

9) Initiatory, orientation, and final survey courses.

10) Facilities for giving intellectual comradeship to the ablest students among themselves and with the faculty.

11) Development of a system of character record and the motivation of character as such.

12) Development of adequate educational personnel service.

13) Technological training with a natural finishing place in the junior college for students of applied science and art.

14) Differentiation of the functions of institutions.

It was exceedingly gratifying to find a very vigorous response on the part of administrative officers and faculties to these suggestions, and I had the satisfaction of seeing extensive follow-up work both in the leading universities and colleges of the country. It has been said that a prophet is not honored in his own country. However, as a member of a committee of the College of Liberal Arts in my own University, I got sympathetic hearing, and this Faculty adopted a program involving eleven of the fourteen projects in my list at that time. I must say that this gave me more satisfaction than similar action taken in other large universities. The resolution read as follows:

> Two general methods of increasing the opportunities for good students have been considered by your committee: (1) the creation of honors courses, and (2) the gradual building up of a series of practices of selection and preferential treatment which may ultimately be made the basis for the awarding of honors.
>
> The committee favors the second alternative and is of the unanimous opinion that legislation in most of these matters should be only of a permissive and encouraging nature rather than mandatory, in order that there may be a natural growth of practices which may progressively justify recognition.
>
> The committee, therefore, recommends that the faculty go on record as viewing with favor, commending for serious consideration, and authorizing experiments which will lead to progressive selection and motivation of good students, and commend in particular the following devices:
>
> 1) The introduction of the use of placement examinations in courses open to freshmen.

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2) Keeping each student busy at his natural level of successful achievement in the first two years, wherever convenient, by
  a) sectioning on the basis of ability; or
  b) providing individual freedom of progress in laboratory and project courses within an enlarged scope of assignments.
3) Continuing the present practice of the Dean's office in interviewing the most promising students at various stages and assigning them to advisers.
4) Permitting appropriate faculty groups, such as divisions of language and literature, biological sciences, mathematical and physical sciences, to publish in the catalogue advisory statements of principles on which students who wish to major in their respective divisions should make their electives the first two years.
5) Publishing near the close of the second year an honor roll based on grades earned in the first three semesters of the junior college.
6) Differentiating between junior and senior college methods of instruction.
7) Providing that, wherever possible, when upper classmen elect courses which are open to freshmen, the work shall be taken in a special section provided for upper classmen.
8) Providing for admission to certain classes restricted on the basis of ability.
9) Giving upper classmen the privilege, in lieu of a certain amount of regular class registration, of doing individual work or voluntary group work either of a general survey character or of the nature of original investigation, at the discretion of the department and with the approval of the honors committee.
10) Granting honor credits to the end that a good student may graduate on the basis of these credits in less than four years to the same extent that he might by taking excess registration.
11) Granting to departments or larger divisions of subjects the privilege of developing and trying experiments with prescribed honors courses in given fields.
12) Providing for the award of honors at graduation in harmony with the above provisions.
13) Providing that a committee on honors, with the Dean as chairman, be established as a standing committee of the faculty.

It is interesting and gratifying to compare this action taken by our Faculty with my "Fourteen Points," and to analyze and interpret the principles in terms of psychology applied to higher education.

During this period I was sometimes accosted by critical men, saying, "What business has a Research Council with these matters?" and my answer was a personal one: "Psychology is one of the divisions of the Council, and the research which I am doing is applied psychology; psychology applied to higher education."

These visits gave me an extraordinary opportunity for gaining acquaintance with the leading educators of the country and for sensing the layout of American institutions, especially in relation to current trends of progress. I think it is safe to say that no other man has met as many faculties or faculty committees in the interest of higher education as I had the pleasure of doing.

A sort of free interpretation of the Gifted Student Project is contained in my book, *Learning and Living in College.* It represents in epitome my present educational theory, which I feel is a rather comprehensive attempt to apply psychology in the organization of higher education.

This traveling in addition to my work at home was very strenuous. On one trip I was eighteen nights in a sleeper, and at one time I made forty-seven after-dinner speeches in forty-six consecutive days, not counting my regular addresses to faculties or conferences. I think of an extreme situation. I was met at the train and taken for breakfast with the Department of Psychology at eight. From ten to twelve I had a conference with the President and Deans. At twelve I had luncheon and conference following with the Law Faculty. At two I met a group of students which had been selected by allowing each Department to send one or two of its most promising students, a very interesting collection. At four there was a meeting of the Faculty of Arts and Sciences, and at six there was an all-university dinner followed by discussion which continued until eleven. To go through all this and then take a sleeper the following night to meet a similar situation, only perhaps not so strenuous, was a severe undertaking; but I was carried by a feeling of enthusiasm and success in the work.

The popularity of the work, making demands upon my time and efforts, might have been my undoing had I not carried my golf sticks and made place for a game at most places. I have played golf in every state of the Union except Delaware and Rhode Island.

For matters of this kind to come at the psychological moment was a great advantage. The country was at this time just awakening to a recognition of the significance of individual differences, and a testing program and service putting out dragnets to discover gifted students in the senior class, in the freshman class, and in the high schools came to be a popular movement. It was at this time that institutions like Dartmouth, Williams, and Amherst were being thoroughly revamped so as to come out entirely different from their traditional character in many respects. The institutions of the Mid-West and Far West were perhaps at that time at the peak of growing pains and therefore receptive to my mission.

Gradually I found, however, that I could not continue this project, and various means were considered by the Council for its continuation, but the matter was dropped because there was no one who was willing to step into the gap and assume leadership as I had done. As I look back upon this, I wish to voice my profound gratitude to the officers of the Council and to the faculties and officers in the institutions I visited for such cordial welcome, and most of all for the thought that they are giving to follow-up work.

As an example of my ability to deal incisively and effectively with follow-up work my "Open Letter to a College Senior" may be mentioned. I had written this I think in 1912, frankly in the interest of the Graduate School of the University of Iowa. An incident indicative of its success was this. Our Board of Education had asked me to search for a professor of the psychology of advertising and business personnel. I made inquiries from a number of people, and among them, Professor Walter D. Scott, who wired, "I have just the man." I took this wire to the President and asked what he wished to do. He said to wire for particulars, and the same day the reply came back, "The man who wrote the 'Open Letter to College Seniors.' "

This letter has been continued in circulation through a number of agencies up to the present date, reaching in some years a majority of the graduating college seniors throughout the country, and I have testimonials which are accumulative showing that it has pointed the career of a very large number of men. Realizing that the time when we can do this most successfully is at the time he enters college, I wrote my "Open Letter to a Freshman," which is enjoying large circulation and is undoubtedly exerting a wholesome influence.

The War Service. Being an ex-President of the American Psychological Association, I was one of those who were called to New York on the day we entered into the War for a conference to determine what psychology could do to help in the war. We met at dinner in the Faculty Club of Columbia University and about nine o'clock passed a resolution providing for the establishment of mental tests for classification in army service. Like the proverbial debating societies, we sat around and talked over the matter until one o'clock, by which time most of us had lost confidence in our proposition. But already the notion that one must not be a slacker had become dominant and no one would move to reconsider. In this spirit we adjourned, standing by our decision but with a solemn sense of fear concerning the feasibility of the undertaking. Later events show the far reaching significance of this step. From that time on I was in the councils of various war agencies and served as Chairman of the Committee on Acoustic Problems throughout the War. The uppermost problem in this field was that of locating submarines. As is now well known, this was done by utilizing certains laws of illusion in the localization of sound on certain principles of binaural phase. It so happened that I had been working in my Laboratory on this for a number of years previous.

During the War we conveyed all the information that could be collected on the laws of this illusion and furnished it to the International Committee. I continued the direction of researches on this problem in the Laboratory, having the apparatus set up on the roof of one of the large buildings without anyone on the campus except the experimenters knowing what was going on. In this I had the able assistance of Dr. H. H. Halverson and other research students. It is a most extraordinary illustration of a scientific turn of events that this illusion, which had been studied by physicists and psychologists from a purely scientific curiosity, should turn out to be the effective medium through which the enemy boats could be located under water.

Perhaps one of the most significant things we accomplished was the demonstration that in a normal group of men selected through the routine methods for listeners in the Anti-Submarine Service, we might find differences in capacity amounting to as much as one to ten. That is, the best in a group of thirty might be able to locate the boat within approximately one degree, whereas another man equally bright, who had also passed for the Service, might not be able to locate it more accurately than within five or ten degrees. Recognizing this fact we were prepared to put on a service to select the listening personnel on the basis of hearing capacity; but at this time, shortly before the armistice, a photographic process was adopted which replaced the human listener and made the detection of the direction of the source of sound purely a physical measurement, which was a distinct advantage.

My other service during the War centered around the developing of techniques for the selection of the most promising students of science in the colleges and universities of the country to be given special training for scientific war service.

Fellowship Boards. I had the satisfaction of being associated with the movements which resulted in the Post-Doctorate Fellowship foundations of various kinds. To me it was a natural outcome of the

Gifted Student Project, and I have no doubt but what the popularity of that project had much to do with the interest in these boards. I have served as a member of the Biological Fellowship Board in the National Research Council from its beginning up to the present time. This board also administers foreign fellowships. I have also served on the Guggenheim Fellowship Board from the beginning up to the present time.

By virtue of these relations and my experience and responsibilities as Dean of the Graduate School, I have had many opportunities to be of service in the selection and encouragement of students for super-fellowships. The expenditure of money for this purpose has been most gratifying. It is rather extraordinary that from one point of view the funds available are really adequate to meet the needs if one takes into account the availability of men who are capable for this kind of super-training in terms of capacity and will to achieve and, on the other hand, the law of supply and demand in the community. The bringing of these post-doctorate research students under ideal conditions as judged by standards for scholarship in the best academic institutions in the country is an epochal event which has very far reaching results. I recall one incident in the Biological Fellowship Board which gave me much pleasure and that was the adoption of the rule providing a bonus for a child that should come to a National Research Fellow. In the Gifted Student Project one of my missions was to urge universities to select material for these fellowships early, as early as possible during the undergraduate work, and organize their training on broad and sound foundations for an intensive career in research.

Engineering Education. The Society for the Promotion of Engineering Education (S.P.E.E.) has, for the last few years, had a Board for the Investigation of Engineering Education, under the leadership of President Wickenden. The Board has been made up primarily of deans of the leading engineering schools, and I was invited to come in as consulting psychologist, jocularly referred to in committee as "the expert." Work on this board was very gratifying to me because it gave me an opportunity to apply in a new field the principles which I had developed theoretically and verified by observation in studying American institutions in the interest of the Gifted Student Project.

I think that possibly an expert, reading the final and full reports from this Board, will recognize in it a majority of the fourteen points which constitute the message in my Gifted Student Project. The key to the program which I had the pleasure of helping inject into this work was, first, recognition of individual differences and, secondly, job analysis of the community as a basis for the organization of training for different levels. Up to that time, engineering education had been very slightly influenced by modern educational methods in Arts and Sciences and the coming in of the Board proved to be the psychological moment for welcoming a review of possible new approaches. Perhaps the most radical recommendation of the board has reference to the matter of establishing two-year courses of technological training for arts and crafts involving some little fundamentals of engineering training but directed specifically to the introduction of training for leadership in all the various branches of skilled labor and for giving dignity to such labor.

C. SPEARMAN

YOUTH AND MILITARY SERVICE

The invitation of the Editor to write one's own "intellectual history," accompanied as it is by a suggestion that this may be helpful to younger men with their lives still to make, is only too alluring. But on looking at the task more closely, one becomes not a little embarrassed. One's own intellectual history, even on its intellectual side, involves much that is very intimate. To publish this abroad seems not only egotistic but even of dubious propriety. And the danger is much enhanced by the fact that the consideration of one's intellectual life, if it is not to be intolerably shallow, inevitably leads over to one's feelings, strivings, and ideals, whereby the intimacy deepens. Yet more, the history of a person is in large measure bound up with his reactions to other persons. It cannot well escape making free allusion to these persons, whether the encounter with them has been in alliance or in antagonism; the autobiographer has to indicate not only his likes but also his dislikes.

In face of these great difficulties and dangers, all I can hope is to hit upon a mean course, where the personal references give as little offense as possible, whilst yet are sufficiently pointed to serve the interests of the reader and aid the good purpose of those who have issued the flattering invitation to write.

After this preamble, I will begin with what may be called the pre-Leipzig period of my life. Before and during my school days, there seems to have occurred nothing worth chronicling except an excessive but secret devotion to philosophy. Little did those set in authority over me, or even my most intimate companions, suspect that under my seemingly exclusive devotion to games and sports of all kinds—and also to lessons, when prizes were offered!—my deepest urge was to probe further into the nature of existence, knowledge, and goodness. So far as I remember, the earliest and most primitive outcrops of such philosophizing went back to about my tenth year. But they did not become very articulate, or even powerful, until five or six years later on. Not infrequently these speculative ventures were accompanied by intense stirring in an ethical direction, such as highly emotional yearnings for the good of all mankind. These seem to have had no appreciable influence in ameliorating my actual conduct. But for the future of my psychology, as will presently be seen, the ethical bent was fateful enough.

From such a beginning one might naturally expect an academic career. The odds seemed to be that I should at the earliest possible date seat myself at the feet of those learned men whose profession it is to expound and solve the sort of riddles that had so puzzled me. As an outward and visible sign of the grace acquired in this way, I ought to have climbed the ladder of university degrees, till I could say, with another, "Heisse Magister, heisse Doktor gar." With good fortune, from being a disciple in philosophy I might even have evolved into a teacher of it. Waywardly, however, I chose another path. Dipping into the philosophical treatises for myself—self-instruction was always congenial to me—I found their solutions to my problems most unsatisfactory. But on then attempting to unriddle the universe by dint of my own cogitations, I was soon obliged to admit that along this way I also made not the slightest progress.

In this quandary, I committed the mistake of my life. Having no vocational advisor to assist me, I gave myself up to the youthful delusion that life is long. The problems which were now baffling me might perhaps, I thought, succumb to ripened experience. Following the illustrious example of René Descartes—not to mention Socrates and Plato—I decided to turn to a short spell of military service. This diversion of activity was, for one reason and another, allowed to spin out far longer than originally anticipated; it lasted until 1897. And for these almost wasted years I have since mourned as bitterly as ever Tiberius did for his lost legions.

The progress made in the course of this period may be summed up briefly. First of all, I became convinced that if ever a genuine advance was to be made in philosophy, it would come mainly by way of psychology. To this latter study, then, I gradually transferred my allegiance. Nor have I ever turned back again (though possibly such a return to my first love may yet arrive).

In this new study I at once found myself up against a rock that has always troubled me since, and probably has troubled most psychologists—the impossibility of reading more than a small fraction of what ought to be read. Restricted as my choice was, I happened to pick out first some of those works which depict all mental experiences as being at bottom nothing more than an aggregate of sensations variously associated with one another. Leaders of this school in England have been Hartley, Hume, the two Mills, and Bain. My reaction to all this view was intensely negative. The ideas and arguments appeared to me astonishingly crude, equivocal, and erroneous. But, even so, my conviction was accompanied by an emotional heat which cannot, I now think, be explained on purely intellectual grounds. The main source of this heat I take to have been—little as I admitted this to myself at the time—of an *ethical* nature. Sensualism and association tend strongly to go with hedonism; and this latter was (and is) to me an abomination.

As for the other psychological writings which fell into my hands about this time, some of them, notably the textbook of Rabier[1], seemed to me—unlike associationism—at least in commendable agreement with common sense. But much more than this was needed to produce such a psychology as would supply all that I was now demanding of it. Only one hope still remained. Rumors had reached my ears that the attempt was being made to study psychology in quite novel manner, namely, by means of experiment, the method which had proved so extraordinarily successful with the physical sciences. Perplexed though I was as to how such a method could be rendered applicable to the mind, I determined to give it at least a trial. But this resolve meant the closing of my military service. Up to that time, I had managed to carry round a small assortment of philosophical and psychological books from one military station to another. But no way occurred to me of carrying about even the most modest experimental laboratory! Perforce, then, I at this juncture resigned my military commission.

PERIOD IN GERMANY

To achieve my purpose, the most suitable place of residence was, of course, Leipzig, where reigned one of the most remarkable men of modern times, Wilhelm Wundt, the originator of experimental psychology. Mental experiments had, indeed, been made even before his time; notably, those on mental span by Nemesius and Bonnet; on "free reproduction" and its subconscious basis, by Galton; on sensory discrimination, by Weber and Fechner. But all these ventures, even those of that master-mind, Galton, had remained isolated. In fact, at this very time some of the greatest authorities were emphatically declaring that these isolated cases were all that the experimental method could ever hope to manage. But then came the wonderful vision of Wundt. He saw and eloquently proclaimed that the method could be applied to psychology throughout its length and breadth. Suiting the action to the word, he moved his abode to the University of Leipzig and there both founded and long directed the earliest psychological laboratory in the world, an institution from which all the countless psychological laboratories of the present day are directly or indirectly descended.

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[1] *Leçons de philosophie, psychologie,* 1888.

To Wundt, accordingly, I now betook myself. And I was received by him with such great and steadfast kindness that gratitude towards him has become one of the dominant sentiments of my life. Together with this feeling due to my long intercourse with him, there continually waxed also a disinterested admiration for his personality. In situations of every kind and description, from delving into the profundities of science to leading the amenities of society, everywhere alike Wundt stood out as the Master.

Bizarrely enough, however, the very matter upon which I was least inclined to admire him without stint was just his psychology. For with him, too, I seemed to find, not indeed the crass sensualism of the associationists, but still a tendency to be pre-occupied with sensation far more than this deserved. He did get beyond it in his various applications of psychology, and also in the latter part even of his general *Physiologische Psychologie* itself. But these trans-sensory parts of his work were never thoroughly integrated with the sensory parts; they appeared rather to have been appended as corrective afterthoughts.

At the present moment I view all this rather differently. The *starting point* in experimental psychology, I now think, consisted very properly in a very exhaustive study of sensation, for the purpose of ascertaining both what it can and what it *cannot* explain; so that Wundt's pioneering work in this direction was really indispensable. Later on, when he had in this manner furnished psychology with a foundation and might have gone on to build up its superstructure, he was too much engrossed in other work to afford us more than some extremely brief but marvelously pregnant general principles. In these he reveals how far he saw into the promised land. But the actual invasion and conquest of it he left to his followers.

In another respect, also, time has taught me to appreciate his great textbook more adequately than at first. It seems to me still quite unmatched by more modern books in the excellent virtue of embodying the chief results of investigation up to date. To this day, I frequently hunt in vain through the most modern writings to find some bit of information, and then turn at last with success to my old friend the "P. P." (The only modern textbook that rivals his in this respect I find to be the *Lehrbuch der experimenteller Psychologie* by Fröbes). In this way, it has curiously happened that during the last score of years, whilst his popular reputation has been continually diminishing, my own estimate of him has only risen higher.

This evaluation may be concluded and pointed with the following personal anecdote. In spite of being the leader of us all in the usage of the experimental method, his other activities came afterwards to press so hard upon him that, by the period of which I speak, he had almost ceased to superintend in person the researches going on in his laboratory. On one of the rare occasions when I did see him enter it and inspect an experimental arrangement, it so happened that all of us—both students and staff—were being perplexed and baffled by some technical difficulties. No sooner did we mention our trouble to him than he with consummate ease forthwith solved it for us.

In general, however, I had to learn the experimental technique, not from Wundt himself, but from his two assistants, Krueger and Wirth (Klemm, to my regret, only arrived just as I departed), and most efficient they were. With Krueger my relations were especially intimate, as is instanced by the fact that we even collaborated together in a small piece of research.[2] How valuable such intercourse must have been to me has been demonstrated by his brilliant career subsequently. Elected to no less a position than that of successor to Wundt himself, he has been able to maintain, and even to increase, the activity and prestige of the Leipzig Psychological Institute. In addition to his other qualities, he was at that time exceptionally broad-minded. In argument I found it easy to come to terms with him on every point except, unfortunately, just the one which he had specially undertaken to champion; this was the doctrine of Cornelius, that "feelings" are "complex-qualities."

Of Wirth, great as has become his subsequent reputation—especially as editor of the *Archiv für die gesamte Psychologie,* I think it deserves to be much greater still. His drawback has been that many people find him hard to understand. The difficulty in his writings, however, is not at all because they are obscure, but because they are so highly condensed. He lightly carries such a load of thought that ordinary people can scarcely keep with him. He puts into a sentence what others do into pages; and into pages, what they do into chapters. But the reader has an easy remedy; he need only give to Wirth's writings time and trouble proportionate, not to their length, but to their weight of content; and then he will find Wirth to be among the clearest as well as the profoundest psychologists of the present day, in Germany or elsewhere.

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[2] "Die Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten." *Zsch. f. Psychol.,* 1906, **44.**

In 1900, my stay at Leipzig was interrupted by the Boer War. I received and accepted an invitation to return to military service, as the Staff Officer for Guernsey, a position of some importance, owing to the dubious attitude of France at that critical period. Two years later, I managed to get back to Leipzig, and this time not alone. For in Guernsey I had found a life-partner, who thenceforward was the mainstay of all my undertakings. I now took the degree of Ph.D. at the University of Leipzig, my major subject being of course psychology, whilst my minor ones were history and political economy.

But, during this second and longer visit, the greater part of my time was really spent on yet another subject, *physiology*. Among the courses which I here found of greatest service was practical work under Hoffmann and Gartner, a year of participation in the work of the nerve-clinic under Köster, and about three years of attendance in the mental hospital of Flechsig. To all these kind teachers I here once more record my great debt. But, in one vital respect, the general upshot of all these physiological studies was gravely disappointing. I had expected that the study of the brain would wonderfully illuminate that of the mind, seeing that the latter was admittedly a function of the former. I did, indeed, receive much further confirmation—if any such were needed—*that* the mental processes was most intimately dependent upon those of the brain. But as to *how* this dependence is effected, here, it seemed to me, physiology was still in darkest ignorance. The physiologists who had claimed to assist psychology were, I concluded, drawing heavy drafts on the future; drafts, however, that now at last such men as Lashley are perhaps beginning to meet.[3]

From Leipzig I proceeded in 1906 to Würzburg, there to study under Wundt's illustrious pupil and former assistant, Külpe; of whom it was commonly said that to know him was a liberal education. If he had a weakness, it was that of being too unselfish. The world would have gained, if he had less completely devoted himself to the service of his students and reserved more of his energy for his own productive work; in particular, if he had afforded himself time to replace his not very happy textbook, published in his youth, by one embodying the splendid researches inspired and directed by him in his later years. My three months' stay at Würzburg were additionally precious to me as the occasion of becoming intimately acquainted with Külpe's assistant, Bühler, one of the living psychologists to whom I feel most in debt.

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[3] See his address to the International Congress of Psychology, New Haven, 1929. *Psychol. Rev.*, 1930, **37**, 1-24.

From Külpe I turned to such an extremely opposite person as G. E. Müller, at Göttingen. The former appeared to view everything, however small in itself, as seen against the universe for its background; lining the psychologist was always the philosopher. Müller, on the other hand, I thought had a narrow outlook. One could almost say that he ran in blinkers. And his philosophical lectures, although he took especial pains with them, never seemed to me to have much bearing on his psychological work, or even to approach this in excellence. But such restricted fields as he did include within his view, and perhaps partly by reason of its very restriction, these he penetrated with an acuteness, thoroughness, and exactitude that, I believe, have never been surpassed. To attend his courses on memory, for instance, or those on color vision, was the finest of lessons, not only in these subjects themselves, but in experimental work generally.

At the same university, that of Göttingen, I had the further advantage of attending the lectures of Husserl, in his way, as great a man as Müller. But their ways lay worlds apart. In fact, the sole thing that seemed common to the two was the inability of each to appreciate the other! To Müller, Husserl's fine analyses seemed to be a revival of the Middle Ages (as, indeed, they largely were, but not necessarily to their disadvantage). To Husserl, Müller's attempt to cope with psychological problems by means of experiments was like trying to unravel lace with a pitchfork. And yet Husserl's own procedure—as he described it to me himself—only differed from that of the best experimentalists dealing with similar problems in that he had nobody but himself as experimental subject.

My debts to other German psychologists at this period must be passed over very briefly. To Stumpf at Berlin I paid an all too fleeting visit. Ebbinghaus I met only casually; but this was enough to make me keenly sensible of my loss in not knowing him better. Incidentally, he was the first leading psychologist to befriend my work on "general intelligence." Of Meinong—to my shame be it said—I knew at that time little more than the name. Among the younger generation I should mention Stern, Ach, and Fröbes, as seeming to me those from whom one had much to learn; an estimate which subsequent experience has more than justified. But as I write these names, a crowd of others surge up into my remembrance with whom also I had both pleasant and profitable intercourse. In fact, did I not make a more or less arbitrary halt somewhere or other, I should have to quote pretty well the entire list of the then members of the German Psychological Society.

Indeed, I find it hard to stop without some words about the German nation in general. To have lived among them for years, to have enjoyed the unstinted kindness and precious friendship of very many of them, to have appreciated their profound views on life and their wise ordering of it—putting out of account the pestilent canker of the doctrine of Nietzsche—to have done all this carries the penalty of always afterwards wanting to match such a life elsewhere.

Work in London

This visit to Germany terminated in 1907 with an appointment as Reader in Experimental Psychology at University College, University of London. It was in succession to that pre-eminent psychologist, W. McDougall, who had previously taught both in London and also in Oxford, but thereafter confined his activities to the latter place. At first, my function was to supplement the work of my colleague, Carveth Read, who held the chair in General Psychology. But this very distinguished writer and lecturer—to the great regret of all who had the privilege of knowing him—retired in 1911. Thereupon, the experimental and general psychologies were united under a single chair, to which I had the good fortune of being elected. This arrangement lasted till 1928, when another change took place, and one of interest for the scientific status of psychology at universities. Up to that time, psychology had been linked with philosophy in a single department, under the joint control of the representatives of the two studies. But now the Professor of Philosophy, Dawes Hicks, retired, and I took the opportunity to press for making psychology a separate department. This I managed to get done, my title then simplifying from Grote Professor of the Philosophy of Mind and Logic to plain Professor of Psychology. This independence would, I hoped, aid materially toward drawing nearer to the physical biological sciences, as physiology, anatomy, zoölogy, botany, anthropology, and pharmacology; also, to phonetics. Incidentally, it may be mentioned that this change in the status of psychology was accompanied by a large increase in its staff and laboratory.

Throughout these various phases of organization at University College, by far the most conspicuous feature of my work was research. Nothing of the kind, as far as I could ascertain, had ever been completed there before. But from 1907 onwards, it has issued in an uninterrupted and ever-increasing stream. A special characteristic in this development of researches has been an extension of the area from which their authors have emanated. Originally coming almost exclusively from the British Isles, they arrived more and more from distant dominions and foreign countries. Among the chief contributors have been Australia, New Zealand, South Africa, Canada, United States, India, China, Japan, Egypt, and the European continent. And the quality of the students has not suffered but improved. Probably the very fact of journeying from the other side of the earth may be taken as an indication of exceptional zeal and initiative. Besides, such long voyages need usually some powerful support, both official and financial, which is only forthcoming for such students as appear likely to distinguish themselves.

In directing the work of these research students, there arises a choice between two opposite types of procedure, a choice that has probably been fateful not only for their scientific output but also for my own. With the one manner of procedure, the student receives every encouragement to select his topic of investigation for himself; and then he is urged to carry it out as far as ever possible on his own initiative and resources. With the other procedure he instead receives throughout much more help; the director of the laboratory suggests to him the topic of research, and then continues to collaborate with him. At first sight, everything seems to speak for the former plan. The responsibility thrown on the student would seem likely to call forth his best endeavors. And his achievement of success under such stern conditions of independence should enhance his self-reliance and his originality. And as for the director, *he* certainly is spared an infinity of pains. But there is another side to the picture. In most cases, the would-be investigator has no marked preference for any topic in particular. Or, if he has one, it is usually based upon ignorance; he does not know what sort of work the research will involve; nor does he realize what sort of results may fairly be expected; indeed, a topic chosen by an inexperienced person is only too likely to fail to yield any significant results at all. And then, when the student is allowed to start working with the maximum degree of independence, the all-too-common upshot is that he wastes an immense amount of time to no purpose, and the result of this is not self-reliance but discouragement. Moreover, even if he does pursue his way with a modicum of success, his interest is apt to be starved by lack of contact with whatever else is going on in the same laboratory.

Moved by such considerations, I have myself preferred an avoidance of either extreme. Whilst always impressing upon the student that he is free to choose his own topic so long as he does so on his own responsibility, I have carefully set forth to him the scientific situation, the lines of research that were likely to prove most fruitful in general, and also the directions that were most favored by the special knowledge and particular conditions prevailing in our laboratory. In this manner, the question as to whether the choice of topic should be made by the student or by the director solves itself automatically; both choose the same thing. Further, the whole work of the laboratory acquires an organic unity. The interest of each research tends to enhance that of all the others. This consanguinity of the different topics investigated brings the students into stimulating intercourse, not only with all the staff, but also with one another. Still more striking is the increase in scientific value of the total output. Each research supplies some particular stone towards the final erection of one general edifice.

An obvious corollary from this manner of working—and in fact the chief reason for my describing it here—is that the credit of the scientific results ought, in fairness, to be more or less pooled. If much that has been published over the signature of my students has really had its source in suggestions from myself, on the other hand, much that has appeared in my own name—indeed, probably a far larger share than I myself have any idea of—really owes its inspiration to them. Hence it is that in giving any account of the results achieved, I prefer to use, so far as literary and grammatical considerations permit, the first person plural rather than singular.

The indebtedness thus acknowledged to my students holds far more still towards my colleagues. Thus, Flugel has been my right hand in the laboratory almost from the beginning. Aveling not only helped there for several years, but even after acquiring his own independent department (at King's College, University of London) has always continued to be the most sympathetic, wise, and intimate counsellor I have ever had. Wynn-Jones also worked with us for several years, before he too obtained an independent post (University of Leeds), and to his insightful and precious cooperation I gladly acknowledge my debt. Bernard Hart I am proud to claim as having at one time been both my student and my collaborator. With Burt also I have had the honor of some formal and much informal collaboration. Finally, in the last year or so, I have benefitted very greatly from the collaboration of Holzinger. All these helpmates I would like to include—though by no means sure how far they would like it themselves!—when in the following pages I write of "we" and "us."

Relations to the United States

At a very early stage of the above-mentioned work in London, my chief foreign interest began to shift over from Germany to the United States. In the former country, partly owing to the political situation, and perhaps still more so to a national idiosyncracy, adequate notice is not easily accorded to scientific work done elsewhere; or not, at any rate, until after the lapse of a century or so. In my own case, for instance, most of those present-day writers who are good enough to quote my work mention exclusively a very small contribution made by me in the German country and language as far back as 1906! Indeed, it has come about that much of the present utterances there on the whole subject of human ability and "intelligence" are painfully suggestive of Rip Van Winkle.

The Americans, on the other hand, besides vigorously cultivating psychology themselves, have been keenly alive to its development elsewhere. Their reaction to the work emanating from us was at first that of opposing it tooth and nail. But opposition, unlike apathy, only helps a good cause; feeble fires may be blown out by the wind, but strong ones are made the stronger. And, as we shall see later on, the chief support for our work, no less than the original opposition to it, was contributed chiefly by Americans.

I cannot refrain from adding my appreciation of the fact that all this scientific intercourse with the States has brought in its wake much personal intercourse also, including three visits to the country. Many are the qualities I have learned to prize there. The very exemplar of hospitality is presented by the whole-hearted welcome with which all Americans—save only the immigration officer!—extend to their visitors. A revelation of the right democratic spirit is afforded by the frank level friendliness with which persons of all positions in life meet together. A veritable lesson in social technique—especially needed on British soil—is furnished by the sunny uninhibited manners with which even strangers "mix." And, under all this light, bright surface, one feels an energy of purpose and an ethical enthusiasm which—far more than their broad lands and countless dollars—render this people the most potent and even the most alarming of modern times.

"Ideo-presentation," A Research That Failed

So far, this story of my work has been oriented by dates, places, and persons; the description has been external rather than internal. But from now on we may proceed more lucidly along the main lines of scientific endeavor, taking each line in turn.

The one to which I devoted myself first of all—even before entering into military service—was adopted in my abhorrent recoil from the sensualistic doctrines. Its fundamental aim was to overthrow the dogma that all thought consists essentially of "images." Any such proposition was in absolute conflict with my own experience, seeing that in my case such images—whether of vision, sound, touch, movement, or any other kind—were almost entirely absent. Nor were the images in my case replaced by anything of a verbal nature; my thinking was mostly not only imageless but even wordless. To the mental stuff that really was the vehicle of my thinking I gave the name of "ideo-presentation." This was intended to distinguish it, on the one hand, from "presentations" in the sense of actually present sensations, and, on the other, from "representations" in the sense of those more or less ghostly revivals of sensations which have been called "mental images."

This "ideo-presentation," then, was the first thing that I ever attempted to investigate. About it, I continued to amass observations in a patient though desultory manner for twenty years. Then came my downfall. Chancing finally upon the works of Husserl, I discovered that in these the cream of my results had been forestalled. So all my accumulated material went perforce to the scrap heap.

Nevertheless, I followed up the fate of the topic with lively interest. For in this fate I cannot but see one of the tragedies of psychology. A long series of researches—notably by Bühler, Ach, T. V. Moore, and Aveling—seemed to demonstrate beyond all reasonable doubt that this ideo-presentation, or as it is now generally called "imageless thought," certainly exists. Even more conclusive has appeared to be the objective evidence supplied by Betts, Carey, Thorndike, Ballard, Martin, and Aveling, which showed that no excellence of images—either their vividness, or their steadiness, or their completeness—had any correlation with excellence of thought. The tragic element enters upon the scene in that all this seemingly overwhelming evidence has produced little or no effect upon those who had been asserting the opposite. The trouble is not merely that the two parties remain obstinate—*this* would be nothing uncommon in any science—but that the grounds they give are so discreditable to psychology; each side roundly accuses the other of fundamental incapacity to introspect! Such an attitude would seem to bar the way even to further investigation of the matter. Another sinister aspect of the controversy is the fact that the taking of the one side or the other obviously depends in large measure upon merely subjective influences. The leaders of the party championing the images are almost always persons endowed with these in rich measure. Conversely, those who deny their importance are usually those who are nearly destitute of them. The evil dominance of suggestion in the dispute is further evidenced by the fact that almost all young psychologists trained in a laboratory where the presence of imageless thought is denied by those in authority are surely led to assert that they, too, can find none in their experience. Just the opposite conclusion is reached by those who are trained in the opposing camp.

This singular deadlock extends its pernicious effects in several directions. To it, for instance, I would ascribe much of the present deplorable confusion in the psychological usage of the term "meaning." For what is usually denoted by this name in sensory perception seems to me to be really little else than its "ideo-presentational" constituent.

Psychology, Ancient and Modern

Whilst others—with such unsatisfactory results—were endeavoring to settle among themselves whether thought is or is not reducible ultimately to a stream of images, my own main activity was transferred to the work of Husserl. Surely, I argued, the great need of psychology is that its present crude descriptions should be replaced by some such finer delineation as that which was being taught by this sage of Göttingen. But, frankly speaking, my efforts in this direction were disappointing. The general re-expression of mental experience in the terms of Husserl—with or without attempted emendations of my own—did show itself to be quite feasible, and even to result in a greater descriptive exactitude. But, besides being much more cumbrous, this new version never seemed to lead anywhere; it threw no helpful illumination, that I could see, upon any of the problems in which anyone is interested.

Baffled in this direction, I tried to console myself with a glance at the psychological literature of earlier times. It would be gratifying, I thought, to see how the present state of psychology—open to criticism as it might be—had at any rate left far behind it the state reached by our ancestors. I expected to smile at their old obsolete psychological notions as one does at old maps, or even old anatomical or physiological books. The result was an unpleasant shock. As regards the sense organs, indeed, the ancients lay far behind us. Of course, moreover, the concept of evolution was much less conspicuous formerly than at present (though by no means absent even in very ancient literature). But for the rest, the older writings, far from having become obsolete, seemed quite as alive as the modern ones. In the English language, for instance, the works of neither Hamilton, nor Porter, nor Brown, nor Stewart, nor Reid, nor even Locke seemed to me to have been fundamentally superseded by those of James, or Ward, or Stout. Similar conclusions were reached about the writings in foreign languages so far as known to me; this included German, French, and Italian.

Even greater was my surprise when I went still farther back and ventured into the psychological literature of the Middle Ages. For, instead of the sterile artificialities which I had been led to expect here, I actually came upon writings—notably those of Thomas Acquinas—in which half of the troubles of modern psychology appeared to have been already met and overcome. To some purpose, after all, had these forebears of ours found their delight, not in contests of cricket or baseball, but in those of debate. We cannot credit them with making any important accumulation of facts, but they did wonderfully clear up the chief concepts and eliminate many dangerous fallacies.

From all this study I set myself to compose a general account of psychology ancient and modern. And this I have been developing ever since. But various circumstances have delayed its publication.

"Noegenetic Laws"

About the same time as these historical studies, I started upon a new positive research in place of the ill-fated inquiry into "ideo-presentation." Here I reaped for the first time some manifest benefit from my schoolboy studies of J. S. Mill. Shallow as had seemed to me his psychology, at least he had graven in my mind the conception of science as founded upon *laws*. I could not but agree with him that these have the office of explaining how observable facts accompany and follow one another; in this way they make these facts amenable to diagnosis, prognosis, and control. If I dissented anywhere from Mill, it was in cherishing the further ideal that such laws should be rational; that is to say, besides stating what goes with or follows what, they should also indicate that this occurs by the very essence of things. Here, then, in fundamental laws it now seemed to me was the thing of which psychology stood in most vital need; these were what it must obtain, if ever it was to become a genuine and progressive science worthy to stand alongside of the physical sciences. Not that this was a new discovery on my part. It had been announced by many psychologists before my time, and even before Mill. Very notable had been the words of Hartley[4] and Hamilton.[5] Still better known had been those in which the most successful of all modern expositions of psychology—that of James—reserved for its final page the formal utterance of its own damnation; declaring that for lack of laws psychology was "no science" but only "the hope of a science."

Despite this reiterated *demand* for the laws, however, only one serious attempt, so far as I knew, had been made to *supply* them. Laws of a sort had been (and still are) peppered freely enough over the pages of psychology. But in only one case had they even a semblance of constituting a *fundamental and exhaustive system*. This outstanding exception had been furnished by the great doctrine of associationism, as first developed in England, and then diffused over into all other countries. But unfortunately just this associationism, as has been said above, was in other respects altogether in-acceptable. The so-much-needed laws, then, were still to find. Here was a problem indeed! A sphere of such alarming magnitude and bristling with such difficulties and dangers, that angels might well have feared to tread it. With a courage largely due to ignorance, I ventured to rush in.

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[4] *Observations of Man*, 1791, p. 1.

[5] *Lectures on Metaphysics*, 1865, Lecture 12.

My enterprise started out from the solidly founded theorem that the associationists had ignored the mental power of *knowing relations.* Bain and others did, indeed, talk at great lengths about what they called "relativity." But this really concerned the relation of percepts, which is a very different thing from the perceiving of relations. The latter would have upset their whole doctrine; since this asserted that mere sensing, associating, and reproducing accounted for *all* knowing whatsoever. Even those psychologists, too, who were most averse to associationism and most favorably inclined towards a much more delicate analysis of the operations of knowing, had nevertheless still accorded to this power of knowing relations an extraordinarily scant notice. Of course they had never been able—any more than anyone else, even the associationists—to avoid *incidental* allusions to this power whenever perception or thought was mentioned at all. For does not an awareness of relation manifestly occur when, say, a child observes that his share of the cake is smaller than his brother's, or when the politician declares that he alone can save the nation? But all psychologists alike seem to have been unable to see the wood for trees. The very fact of their cognizing relations so incessantly and ubiquitously had led them to take this event for granted, instead of putting it into the forefront of their psychological theory.

From relations it was easy enough for me to ascend to the considerations of "forms." For all things, physical or mental, are said to possess a form just insofar as the relations between their constituents fall into an orderly system.

Further study showed that the operation of cognizing relations or forms admitted of certain interesting variations. Conspicuous here is the contrast between the two types of procedure which have been called "synthetic" and "analytic," respectively. The former has also been described as a movement from parts to wholes, the latter, as one from wholes to parts. An instance of the former or synthetic case is when, after having looked through a list of names, we suddenly become aware that two of them are the same; in this way the two names, from having been comparatively isolated from one another in our mind, are put together or synthesized into a pair.

The reverse case might occur if the two identical names were shown to us side by side originally; these we might perceive as a pair to begin with (seemingly, at any rate) ; then either name could be considered in itself and apart from its duplicate, by means of subsequent analysis. When the two things are seen in relation from the very beginning of the perception, they and the relation appear to be more blended together; the relation is more vividly perceptual (*anschaulich*). To many psychologists, the distinction between the two types of procedure might seem to be otiose and futile. In truth, however, a failure to grasp the connection between these two types has been the cause of much of the present trouble in psychology (notably, with reference to the various schools of Gestalt).

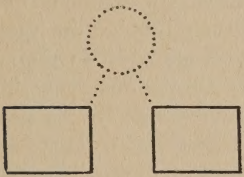
In this fact of cognizing relations, familiar to me long before even visiting Germany and, indeed, already constituting one of the main sections of my youthful work on ideo-presentation—I became more and more certain that I had in my hands at least *one* of the fundamental laws which I was seeking. But I was obliged to admit that it could not be made to cover the domain of knowing completely; no, not even when all kinds and degrees of associative reproduction were added to it. For the life of me, however, I could not discover what was still missing. Scarcely a day passed but that at least a few moments were given up to wondering as to what could possibly be the required supplementary law or laws?

Then at last—as sometimes a long-standing and all-obscuring fog suddenly lifts its veil and discloses a smiling landscape—so my mental view most unexpectedly cleared up, in a moment, it revealed that which I had for so many years been vainly seeking. The credit for the revelation was due to a chance traveling companion in a railway train. He was the commander of a training station for submarine officers. The Admiralty had assigned to me the task of supplying him with useful suggestions from the standpoint of a psychological expert! With this aim, I had told him that in submarine situations, as in all others, the essential requirement was that the situation should by virtue of reproduction evoke in the person's mind an effective plan of action. To do this, I said, nothing was needed but to establish an association between the two, perceived situation and effective plan of action. About the perceiving of situations, I had plenty to say; the whole cognition of relations and forms was relevant. But about reacting, I was only on the level of the associationists, reflexologists, behaviorists and *hoc* *genus omne.* And from this position I was at once ignominously dislodged by my Naval friend. For he answered back that in submarine warfare the situations were very often *new,* so that the plan of behavior would have to be new also; any mere reproduction of what had been done on previous occasions could only lead to disaster. Then at last in my quandary the long awaited "brain wave" arrived. My friend was obviously right, that the plan of behavior could not well be *merely* reproductive. Nevertheless it appeared to me that something even in the most original behavior really was reproduced from former experiences; namely, the relations, form, or method. If this was so, then old relations applied to a new situation were capable of generating a new plan of behavior. Here, in such generation of new behavior, was indeed a wonderful achievement! How broad, I asked myself, was its scope? Was it no more than an isolated curiosity? Or did it possess a large and important field of application? Could it by any possibility extend over the whole range of cognition, and thus supply a law coordinate to that of educing relations? All these thoughts ran through my head in the stretch of time that it would take you to light a match. I no longer remember how I replied to my companion, but only that I ungratefully longed to be alone. I had come upon a treasure chest and was impatient to see what was in it.

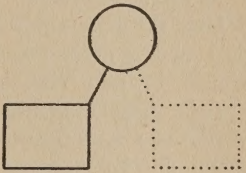
Thereafter my work passed on into a very different phase. This lasted about as many years as the preceding one, and was far more strenuous. But it was no longer an almost hopeless battering against immovable barriers. Instead, there were now fresh advances made every day; and, to boot, with perfect confidence that this would be so.

One great task to be executed was a fundamental comparison of this newly founded process with the so long familiar one of cognizing relations. Profound were discovered the resemblances to be; but no less profound, the contrasts. A simple example of the two processes, old and new, may be taken from music. Sound the notes *c* and *g*. Any person with a musical ear can perceive that the interval between them is that of a "fifth"; this is the old process, that of perceiving relations. Now, sound any note and ask the person to imagine another note a fifth higher. If musical, he will be able to do so and this will be the new process of which we have just spoken. From sensory perception let us go on to thought. An example of the old process of cognizing relations is at once given to us by the well known mental test, where the testee is presented with the names of two qualities, say "good" and "bad," and is asked to state whether they are the same or opposite. For a parallel example of the new process, we can use the same test with a modification; the subject is now given one quality and a relation, "good" and "opposite"; and he is asked to state what other quality stands in this relation to the first or given quality.

Symbolically, the two processes may be indicated by the Figures *A* and *B*, respectively. The drawn-out lines denote the elements given originally, whereas the dotted lines denote the additional element evoked finally. The circles stand for relations, whilst the squares are the things related.



A. Educing of Relations



B. Educing of Correlates

For convenience in describing these two processes, we may remember that in ancient literature any items in any relation to each other were often called the "fundaments" of this relation. If our first process, then, be taken as that of educing relations, the second can be called the educing of correlative fundaments, or, more briefly, the educing of correlates.

At this point, the whole investigation entered upon yet another phase. Firmly as might be established these two processes with their corresponding laws, there was nothing yet to indicate that they were laws of the kind really needed by science; namely, such as to constitute a system which exhaustively covered a large and definite domain of mental events. In what domain, I had to ask myself, lay those two laws which I had obtained? Clearly, they both dealt with the *generating* of items of knowledge not (necessarily) known to the person before (the dotted parts of Figures *A* and *B*). Furthermore, this knowledge seemed to be gained by *self-evidence*; for instance, whoever understands the meanings of the words "good" and "bad" can thereupon without any further evidence, assert that the two qualities are opposite to one another. In place of "self-evident," one may call such knowing without further reference "intuitive," or "noetic" (from the Greek νοῦς). As a word to combine these two characteristics of "generative" and "noetic," our laws could be called "noegenetic." Our question, then, resolved itself into asking whether or not our two laws accounted for the entire domain of noegenesis.

The answer was a decisive negative. Both the one and the other law only showed how the mind can pass from two elements already given to a third and new one. Obviously, some explanation was still required of the elements thus assumed to be already given. At least some additional law had still to be discovered.

Without attempting to describe here the course taken by this further investigation which showed itself to be needed, I will plumply give its eventual upshot. This was the following additional process and law, third in order of discovery, though first of the three by logical and genetic priority. Expressed crudely in familiar language, it is that "the mind tends to know its own experience." As to the many difficulties raised by this law (e.g., by the problem of sensation) and the many illuminations shed by it (e.g., on the problem of subconsciousness), reference must be made elsewhere.[6] For the purposes of our present account, the great question was as to whether—with this addition—the noegenetic system was at last complete. Was it so complete as to include *every* case of evoking any new item into the mind and *every* case of knowing anything with self-evidence? The conviction at length forced itself upon me—the most emotional event throughout my intellectual history—that this was so; that *nothing* more was needed. Look where and when I would, not a single case could I discover of any new item entering into the content of mind, or of any self-evidence occurring, that could not be expressed in terms of the three preceding laws. Nor, so far as I know, has even an attempt to demonstrate any such exception been seriously made by anyone else. This, then, was the supreme merit that I ventured—still with much diffidence and many reservations—to claim for the three laws. It was not that any one of them (even the eduction of correlates) possessed any peculiar merit by itself. It was that *the three conjointly constituted a system covering exhaustively the whole domain of generation and self-evidence* (as explained above).

Although with the establishment of these three laws my mission had achieved its climax, there still remained much to do; but from now onwards the sailing was in smooth waters. One indispensable further task came from the fact that the said three laws suffered from the limitation of being only qualitative; they stated the conditions under which there is a tendency for the knowing processes to occur; but they did not attempt to set forth quite another matter, the conditions under which the tendency becomes actualized. In other words, this set of qualitative laws had to be supplemented by an additional set of *quantitative* ones. These latter, fortunately, gave very little trouble. In a single afternoon the whole set could be formulated as a provisional draft. And, to my own surprise, this first draft has never afterwards needed any change.[7] In all, the laws were five in number: those of constant output, of retentivity, of fatigue, of conation, and of primordial potency. But for a description of these reference must be made elsewhere.

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[6] *Nature of 'Intelligence' and Principles of Cognition.* (2nd ed.) 1927, Chap. 4.

The whole noegenetic theory was complete early enough to present in my academic courses of 1920. Another couple of years served to work through it critically several times and also to induce others to examine it. In 1922 it was published by Macmillan (London) under the title of *The Nature of Intelligence and the Principles of Cognition.*

Herewith the tale of the noegenetic theory might seem to be ended. If we grant that the system of laws is true and that it is complete, what more remains to say? There remains, at any rate, that it should be scientifically useful. This last requisite, it will be remembered, was the rock upon which my attempts to follow along the lines of Husserl's work had come to wreck. And, on the publication of my own work, a friendly critic, Carveth Read, at once told me that his high estimate of it was subject to two reservations. The first was ironical. He wrote: "How the Principles work out will surprise everyone and delight the disinterested. It will not delight everybody." But his other qualification was to the effect that, in the long run, the work must stand or fall by its degree of scientific fruitfulness. Another and less friendly critic carried this second reservation farther still; he declared outright that the Principles might possibly have academic or logical interest, but for practical purposes they were foredoomed to be useless.

That, however, was something to be decided only by experience. And as a matter of fact, it was decided in just the opposite way.

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[7] *Ibid.,* Chap. 9.

A very large number of researches undertaken since the enunciation of these laws have owed to them both original inspiration and eventual success. To see how such inspiration actually arises, take as example the investigation of "error." The three noegenetic laws are, by their essential characteristic of "self-evidence," debarred from directly causing error of any kind or degree. And as these laws claim to include all entering of ideas into the mind, save only that due to reproduction, the inevitable inference is that all errors consist essentially in reproductions. But from this there can be drawn many further conclusions. For instance, one can infer that, by taking the various manners in which reproduction occurs, one will obtain a complete enumeration of the sources of error. All such theoretical inferences supply material to verify by experimental research. And the actual result (with Bradley and myself) has been to corroborate them very fully.[8]

Among other cases where research has been mainly inspired by the noegenetic system of laws, we may quote in particular those of Edwards on memory, of Fowler on conception, of Gopalaswami on motor learning, of Laycock on adaptability to new situations, of Menon on reasoning, of Simmins on learning foreign languages, of Shendaker on formal training, of Cox on mechanical ability, of Line and of Fortes on intelligence in sensory perception, of Hargreaves on imagination, of Hamid on mental tests, and of Seymour on the perception of the blind.

To such researches done in our own laboratory must be added many others similarly inspired elsewhere. Topmost among these towers the recent book of Aveling on *The Psychological Approach to Reality*.[9] And, besides all these definite inspirations of particular researches, comes what is more significant still; this is the manner in which the noegenetic theory has been the means of developing and illuminating the whole doctrine of mental abilities, to which we will shortly turn. On the whole, then, I do venture to claim that this theory has already made good in the supreme test of scientific fruitfulness.

A word may be appended as to how the theory has been received by the general psychological public. By a considerable number of leading authorities it has been accepted with conviction and even with warmth. Much commoner, however, has been the prudent attitude of "waiting to see how the cat will jump." The claims made were so large that most writers preferred not to commit themselves until they had heard what others had to remark. As for actual antagonism to the theory, this seems to have been almost confined to two sources. One of these has been the Berlin school of Gestalt. This opposition seems to be of little importance, seeing that it has been based, in part upon merely verbal confusions, and in part upon the frankly declared policy of this school to be "not interested" in anything but their own work. The other line of opposition is very different and has been taken up by many eminent psychologists of very different schools. It is directed specially against the concept of "energy" being applied to mental events to explain the constant output. But even this resistance to the theory is much less fundamental than it seems; for it has greatly overrated the degree that the concept of energy really enters into the theory. In truth, this concept has been introduced only as a convenient *working hypothesis.* It could be cut clean out of the picture (as could, indeed, physical energy out of physics) without thereby altering one iota of the theory otherwise. As regards the desirability of thus cutting it out, however, the situation has just been wonderfully transformed. Hitherto, the most solid-seeming objection to it has been its conflict with the current view of the physiology of the central nervous system. But this very view would appear to have now received its death blow from the revolutionary work of Lashley. In its place has come a new view, wherein the concept of a psychophysical energy, far from being rejected, is imperatively required.[10]

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[8] *J. Gen. Psychol.*, 1928, **1**.

[9] Publ. Univ. of London Press, 1929.

THEORY OF FACTORS: "G" AND "S"

Here, then, in thus providing psychology with a system of fundamental laws, has run the main current of my life-work. But almost the whole time there has been flowing alongside of it, and eventually has merged with it, another stream of but little less importance. Between the two there is this difference, however, that, whereas the one just recorded ran its course and gathered its strength within the quiet communion of our own laboratory, the latter of which I have now to speak brawled along noisily enough. For it was published abroad in a series of writings spread over a quarter of a century. It thus presented an ample target for criticism—and received this without stint!

The beginning of this second line of activity goes back to about

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[10] See footnote 3.

1901, during my stay in Guernsey and England, between the first and the second visits to Germany. One day, inspired by Galton's *Human Faculty,* I started experimenting with a little village school nearby. The aim was to find out whether, as Galton had indicated, the abilities commonly taken to be "intellectual" had any correlation either with each other or with sensory discrimination. The intellectual abilities I measured by the children's school marks in various subjects; the sensory discrimination, by a musical "dichord" of my own contrivance.[11] The reply of the experiment was prompt and decisive; all the mental powers measured did obviously correlate with each other in considerable degree.

But hastily as I had embarked upon this investigation, I fell to brooding long over the results. Not satisfied with noting that the different abilities correlated considerably, I wanted to know *how much.* With great labor, I evolved an elaborate theory of "correlation coefficients" by which the degrees of correlation could be definitely measured. Then, too late, I began to search for previous literature on the subject and found that the greater part of my correlational theory had already been obtained—and much better—by other writers, especially by Galton and Udny Yule. Here again, then, a great deal of work had been wasted and much believed original discovery was, as such, regretfully scrapped.

My belated consultation of previous literature provided me with another surprise. Among those who had entered the field before me—although I knew it not—has been Cattell. This investigator had not only, unlike myself, made himself acquainted with the newly discovered correlational coefficients, but had even pushed so far as to apply these to mental abilities. But, to my amazement, I found that his results had been just the opposite to mine; for with him the different abilities did *not* appreciably correlate with one another. Had I seen his work earlier, I should certainly have thought the matter disposed of and should never have started my own work in this direction.

Since the conflicting results were there, however, they had at least to be explained. And after much pondering over them, I had at last a happy thought which embodied itself in the concept of "attenuation." This means that the correlational coefficient between two abilities (or other variables) suffers a spurious decrease of apparent size from the (random) errors of measurement involved. A method was devised for determining the amount of this spurious decrease, so that allowance could be made for it. The upshot was to show that such decrease was quite sufficient to account for the lowness of the coefficients obtained by Cattell; and in this way the seeming contradiction between us was happily dissipated.

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[11] Sold by Zimmermann, Feinmechaniker, Leipzig.

But this same discovery of "attenuation" had another and less pleasant consequence, an embroilment with Karl Pearson. On the basis of correlation coefficients, he had just previously announced that mental and physical characteristics were inherited in equal degree. But on my now making due allowance for attenuation, it turned out that his results, *if* they could be trusted otherwise, really proved that the two kinds of characteristics must be inherited very unequally! But then his results were shown to suffer from another further disturbance no less momentous, which was called that of "irrelevant factors." This second disturbance happened to act in the opposite direction to the other one, so that Pearson's two great errors conjointly led to an approximate truth.[12] Unfortunately, these rectifications of Pearson's results would seem to have been taken by him (unlike Cattell) in an unfriendly spirit; and to this may be attributed much of his action ever since.

To go back to my little experiments in the village school, not only were my correlations large, but their magnitudes were noticed to have systematic inter-relations. At first, this system used to be described as "hierarchical," because it was such as to allow the table of correlations to be arranged with the highest values in one corner and with the other values regularly decreasing in both horizontal and vertical directions. Later and more exactly, the system has been called "equiportional."[13] At the present day, the most usual way of indicating this same system is by saying that the "tetrad differences" tend to be zero.[14]

The next thing, obviously, was to verify this system by experiments in several other schools. I was then faced by the problem of explaining it. And here another happy thought came to the rescue. Aided by the concept of attenuation, proof could be furnished that such a system must needs occur whenever each of the abilities at issue is the compound result of *two factors,* of which the one is common to all the abilities, whereas the other is specific to each different ability. Herewith was born into the world an extraordinary source of discord and labor, but also, let us hope, of progress. It has been called the "Theory of Two Factors"; or, in more general terms, that of "Factors."

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[12] See *Amer. J. Psychol.,* 1904, **15**, pp. 72 ff.

[13] See Dodd, "The theory of factors." *Psychol. Rev.,* 1928, **35**.

[14] See Spearman and Holzinger, *Brit. J. Psychol.,* 1924, **15**.

The vital importance of this theory began to be disclosed when a further advance was made. As so far mentioned, evidence had only been brought that the analysis of each ability into the two factors was theoretically *possible.* There soon followed a device by which the analysis could actually be performed, and thus *the general factor could be measured.* This could be done, it was shown, simply by measuring promiscuously any large number of different abilities and pooling the results together. In such a hotchpotch of multitudinous measurements, the specific factors must necessarily—since they vary randomly from one measurement to another—tend in the average or mean to neutralize one another. Whereas the general factor, being in every measurement just the same, must in the average more or less completely dominate. Accordingly, the average (or other central value resulting from the pool) must approximate towards being a measure of the pure general factor. In such wise this principle of making a hotchpotch, which might seem to be the most arbitrary and meaningless procedure imaginable, had really a profound theoretical basis and a supremely practical utility. To emphasize that such a measurement of the general factor by means of a hotchpotch was the chief result of my whole investigation, this was entitled " 'General Intelligence' Objectively Determined and Measured."[15] And as the publication took place early in 1904, I cannot agree with the statement made by Kelley, that the concept of "general intelligence" as something measurable was first put forward by Binet, and in the year 1908.[16]

With this publication, the fat was in the fire. For the doctrine reigning almost unchallenged at the time was that of mental "faculties." Typical among its champions, Binet had long been busily measuring such faculties as "imagination," "memory," "attention," and the like. Altogether remote from him lay any such idea as that of a measureable "general" intelligence. And still more remote was any such idea from the only other doctrine then at all widely held; this was the view of Thorndike, that the mind possesses an infinite number of abilities all mutually independent.

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[15] See *Amer. J. Psychol.,* 1904, **15**, pp. 202 ff.

[16] *Année psychol.*

Now, the reactions of these and other psychologists to the new Theory of Two Factors, and to its practical "corollary of the hotchpotch," were strange enough. Most important was that of Binet, who theoretically continued to profess his old doctrine of faculties, but tacitly and practically adopted the hotchpotch procedure, utterly discordant though it was with his cherished faculties. He and Simon incorporated this hotchpotch procedure in their celebrated scale of tests published in 1905; this was composed of a great many promiscuous tests and was said to discover the subject's "level," which is only another name for his mean result at the different tests. The instant success of this scale overwhelmed all opposition. Even Thorndike could not stand out against it. He, too, indeed, seemed to hold theoretically to his formerly professed doctrine (of independent abilities), but practically he threw himself into the construction of scales on the hotchpotch principle. As for the rest of the contemporary psychologists, who did not follow the theories either of Binet or of Thorndike—and perhaps neither knew nor cared what theory they did follow—these, too, in practice at any rate acted in the same way. There was thus the curious spectacle of everyone enthusiastically adopting the hotchpotch procedure, and yet no one making even a pretence at understanding why he did so! Worse still, those of them—belonging mostly to the school of Thorndike—who did concern themselves with the Theory of Two Factors tried to show that the hierarchy *failed* to occur; they were naïvely unconscious that if it really failed, then all their testing was without any foundation.

This amazing situation lasted until as late as 1914, when it was made even worse by further opposition to the Doctrine of Two Factors on exactly the opposite ground! Whereas the previous objection had been that the hierachical arrangement did not actually occur, Thomson now announced that ordinarily it could not help occurring on purely statistically grounds by the very nature of correlational coefficients; it therefore could have no real significance.

For my own part, I was all this time torn between different urges. Naturally, I wanted to take up arms to defend the Two Factor Theory against these attacks upon it. But also I longed to push on further and explore the vast new field to which this theory had opened the way. And yet a third alluring task was to join in with all the other psychologists who were exploiting the hotchpotch scales in actual practice, educational, industrial, and medical. This last kind of work, however, I temporarily renounced. Among other reasons, the practical application of tests necessitates their standardization; and standardization spells scientific stagnation. With the two other tasks, I was very busy, producing between 1904 and 1927 no less than twenty-two papers on the topic. Several of these were done in collaboration with colleagues, notably Krueger, Hart, and Holzinger. There must be added still more numerous researches carried out by my students, conspicuous names being Abelson, Bernstein, Carey, Dawson, Davey, Gopalaswami, Hamid, Hanlin, Hargreaves, Kay, Lankes, Magson, McCrae, McQueen, Perara, Phillips, Sleight, Slocombe, Strasheim, Webb, Wild, and Wohlgemuth.

At this stage of affairs, our efforts were blessed with a singular good fortune. Among the worst evils in modern psychology is that its two halves, called "general" and "individual," respectively, have been irrationally and disastrously divorced from one another. Now, we too had developed the two halves in mutual independence; our individual psychology had issued from the Theory of Two Factors; our general psychology, from the laws of noegenesis. Would the two remain, as had happened everywhere else, disconnected from one another, or even irreconcilable? To our intense satisfaction, the reverse occurred. The two streams of investigation, although so long pursued by us independently, proved in the end to be each other's mutual and even indispensable supplements. By far the greater number of our illustrations and demonstrations of the noegenetic laws have been furnished by the very mental processes used for testing individual differences. Conversely, the nature and significance of these mental tests have derived their fundamental explanation from the noegenetic laws. In particular, *the "g" which is more or less accurately measured by every hotchpotch of the tests (whether those of Binet or any others) reveals itself to consist really in our noegenetic ability to educe relations and correlates.*

It only remains to add that, as the proverbial drops eventually wear down even stone, so our unremitting labor for a quarter of a century seems now at last to have made some impression upon even the most "hard-boiled" of our opponents. Thorndike himself—an adversary as tenacious as he is courteous—in his latest and most complete exposition of the topic has, I venture to suggest, adopted tacitly the whole general outline of the Theory of Two Factors, including even its mathematical equations.[17] And Truman Kelley has in his latest book—departing widely from that which he published only two years before—gone so far as to corroborate the Theory of Two Factors in all his chief results.[18] The climax was reached last July, when an informal conference held at Chicago to inquire into the chief controversial matters in the theory broke up after its fifth sitting on the ground that there was no longer any fundamental difference of opinion!

There still remains, of course, an indefinitely large field for further research on the subject. But this has now become such as to admit of free cooperation. The old painful and unprogressive state of affairs, where all the investigators were lined up in opposing arrays, has at last become avoidable, and is, in fact, rapidly disappearing.

OREXIS AND "W"

There is one more line of investigation to be recorded here, as having absorbed much of our interest and energy. The lines mentioned already have been, as it is technically called, "cognitive"; that is to say, they have dealt with processes that belong to the domain of knowing. This leaves still to consider those which belong instead to feeling and striving; or, in the conveniently single word re-introduced into psychology by Aveling, "orexis." For the first few years of our work in London, we left these orectic processes alone; not by any means because they were taken to be less interesting than the cognitive ones, but because their investigation seemed to be logically posterior. The study of knowing—despite what has been reiterated by some psychologists to the contrary—does admit of pursuit up to a certain point without much reference to striving; but to consider striving without knowing is at once an absurdity. Moreover, most of the then prevalent orectic psychology appeared to us to be of the "green table" or even journalistic order; unsatisfying to those who would base all scientific psychology upon definite observations and experiments.

But eventually the success that had attended our new statistical tools when applied to the cognitive processes heartened us to venture an application of them to the orectic ones also. Accordingly, when Dr. Webb asked me to suggest a topic for his doctorate, I drew up a very elaborate scheme of orectic traits and advised him to get these estimated for persons in certain institutions (where he had peculiar facilities for so doing) and then to work out the correlations between them. On the scale outlined, the task was of appalling magnitude. Yet it was carried through, and in such a masterly manner as could not, I believe, have been surpassed by any other investigator I have ever met. The results obtained by him suffered from their very richness; his correlation table was so immense as to bewilder one; it may be likened to a gold mine of which as yet only a small corner has been exploited. But even this much has been enough to produce a momentous theorem, namely, that here, in the territory of orexis, there exists another general factor. Webb designated this as "w." As to its real nature, it has provisionally been taken to be that which has given rise to the popular concepts of "will" and "self-control."

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[17] *The Measurement of Intelligence.* New York: Columbia Univ., 1927. See also *Brit. J. Psychol.,* 1927, **17.**

[18] *Crossroads in the Mind of Man,* 1928.

Not only did this excursion from the region of knowing to that of character begin to open up the latter region also; it had unexpected success in relating the one region to the other. For instance, the discovery was made how it happens that, whereas the intelligence measured by "g" is of one kind only, popular opinion distinguishes several different kinds; as "profound intelligence," "quick intelligence," "common sense," or "originality." Such diversified abilities, it now appeared, were really nothing more than combinations of one and the same "g" with differences in character. Another leading discovery about character was its unexpected connections with mental inertia or perseveration, and this direction of research has quite recently had some extraordinary further developments.

Another sequel to the work of Webb was a great advance in the mathematics of factors by Maxwell Garnett. Originally, the practical use of his new formulae did not go beyond submitting Webb's data to reconsideration. But at the present moment, after a lapse of several years, there seems every chance of their usage becoming very general. For the recent conversion of psychologists to the Theory of Two Factors in the case of abilities has created a widespread interest in the possibility of applying some such statistical treatment to traits of character also, in order to rescue this chapter of psychology from its present chaos of details. Quite a number of prominent psychologists have written to me in this sense during the last few months.

MINOR RESEARCHES

Besides the three preceding currents of investigation which ended by so happily flowing together, as also the early work on "ideo-presentation" which terminated in a fiasco, and the historico-critical study which at present only awaits its final touches, I have from time to time been led into several other researches that had for me but subordinate or temporary interest. They may here be disposed of in very few words.

A large proportion have had to do with the perception of space. I chose an aspect of this for my thesis towards the Ph.D. at Leipzig; not because it presented peculiar attractions for me, but rather because it presented fundamental difficulties with which I wanted to grapple before proceeding further.[19] Among subsequent researches in the same general field, one attacked the pathological and physiological side, the material being supplied to me by the kindness of Flechsig.[20] Several other small researches on visual (especially binocular) spatial perception arose in connection with the war.[21] The same may be said of our work in London on nyctopsis and on binaural localization.[22] Another line of inquiry, which at first lay outside our principal investigations—though subsequently it, too, found its chief support in the theory of noegenesis—was the investigation of "formal training"; conspicuous in the early work on this was that of Sleight.[23] Yet another group of researches coming under the present rubric was furnished by those which were almost purely mathematical and therefore were undertaken by me, not for their intrinsic interest, but for the purpose of assisting other work. Here may be counted, for instance, the method of "constant stimuli" for measuring thresholds by means of averages instead of medians (which latter had been used previously by Müller and others).[24] With or even without modifications, this method would seem to have been adopted widely, though tacitly. Another mathematical tool which has rendered better known service was supplied in the formulae for calculating the correlations between sums or differences.[25] This included, as a special case, what has become popular under the strange name of the "Spearman-Brown prophecy formula." Yet another well-known mathematical contribution was the calculating of correlation coefficients from "ranks."[26]

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[19] "Die Normaltäuschungen in der Lagewahrnehmung." Wundt's *Psychol. Studien,* 1906, **1**.

[20] "Analysis of 'localisation' illustrated by a Brown-Séquard case." *Brit. J. Psychol.,* 1905, **1**.

[21] "Visual requirements of aviators." Trans. *Ophth. Soc.,* 1919, **39**. "Psychology of vision in health and disease." *Trans. Ophth. Soc.,* 1921, **41**. Other researches not published.

[22] See Flugel, *Brit. J. Psychol.,* 1920, **11**; 1921, **11**. Also Wynn-Jones, ibid., 1921, **11**.

[23] *Brit. J. Psychol.,* 1911, **4**.

[24] *Brit. J. Psychol.,* 1908, **2**.

REACTIONS TO CONTEMPORARIES

To conclude this account which I have been invited to render of my "intellectual history," a few words may be added as to how far and in what manner my work has been influenced by that of others, especially contemporaries.

On the whole, owing perhaps to some streak of perversity on my part, this influence has almost always been small, or even negative. The effect of the doctrines which I encountered was not so much suggestive as counter-suggestive. Doubtless, I really absorbed a great deal from the writings of others with little or no awareness of so doing. But the conscious effects, at any rate, were usually to stir up a spirit of opposition. Unfortunately, the converse seems also to have occurred. In spite of writing, as I myself believed, in the blandest of tones, the effect upon readers has often been called provocative; so that my literary life seems to have been one long fight. And although every main point has now at last, I believe, met with at any rate tacit acceptance, the same result could have been obtained by a more diplomatic pen with a quarter of the time and trouble.

Some portion of my aggressiveness may be traced to the early establishment in me of a "complex" against the doctrines of sensualism and associationism. Originally, as mentioned above, my moral indignation was evoked by the classical associationists, Hartley, Hume, the Mills, and Bain. But on turning to my contemporaries, I found several still following in much the same lines. A conspicuous place here I assigned to Thorndike. Like Hartley, he appeared to me to have started his psychological career by basing himself upon what he took to be "the real facts of the constitution of the nervous system."[27] But, to me, the facts he had in view appeared no more adequate to indicate the nature of the processes of the mind than, say, the color of a book could serve to reveal its literary content. This basis—to me so absurdly inadequate—induced him, just as it did Hartley, to place his faith in sensualism, associationism, and mental atomism. For him, the mind—like the brain as he conceived it—was composed of infinitely numerous minute elements connected together by associations, now presented under the name of "bonds." Such a beginning seems to me to have hampered his psychology ever afterwards. That, in spite of this, he should have achieved such great work as he has done often made me wonder what services he might have rendered to psychology had his early conditions been more propitious. The reflexologists, as Bekhterev and Pavlov, disturbed me in less degree, since in them I had never expected to find psychology anyway.

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[25] *Brit. J. Psychol.,* 1913, **5.**

[26] *Brit. J. Psychol.,* 1906, **2.**

[27] *Elements of Psychology,* 1905, Preface.

This brings us to the school of "behaviorism." My original reaction to this was to regard it as no more than a South Sea Bubble. John Watson and John Law, I asked myself, which of these made people lose their senses most? But I have since noticed that, whilst the title has gained an ever-widening adherence, the extravagancies that originally brought it into fame have been silently discarded. After this fashion, it has been becoming more and more true, though at the price of containing less and less that is new.

Not to behaviorists of any kind, however, nor to reflexologists, nor even to Thorndike, did my negative reaction reach its highest intensity, but to that very remarkable and, I believe, ill-fated man, Titchener. Sensualist and associationist I should call him, like the others. But, in addition and foremost, he has been the author and champion of a peculiar method of introspection. He seems to have been led to it by his very virtues. He was determined to exclude as untrustworthy every alleged mental event that would not stand the teststone of being introspected clearly and steadily. Such a methodological principle sounds admirable, but in point of fact appears to have worked execrably. By dint of the over-carefulness, the introspection degenerates into a sort of inward staring. This is just the kind of introspection that really does distort the mental content and thus merit the otherwise much exaggerated charges made against introspection by Comte and others. In particular, this staring has the tragical property of banishing from view almost all mental events save only sensation and perhaps hedonic affection. The evil effects of this unfortunate experimental attitude were with Titchener aggravated by the fact of his having himself unusually brilliant and abundant mental "images." For these would seem to obliterate the more tenuous imageless experiences, much as sunlight dims the moon and effaces the stars. The ensuing harm was rendered still worse by his doctrine of "structuralism" (a discussion of which I will reserve for another occasion). Had any ordinary man done these things, little would psychology have recked of them. But Titchener had such extraordinary abilities and such an impressive personality, that these doctrines of his seem to have blocked the advance of psychology for many years. And even when they themselves eventually collapsed, it was only to give birth to reactionary extravagancies nearly as bad. Among these may be counted the initial excesses of behavior, as also a part of what passes under the name of the doctrine of Gestalt.

This brings us to a very different influence in contemporary psychology, not associationism, but its sworn enemy, Gestaltism. First came what has been called the Austrian school, which emphasized the importance and peculiar nature of the act of perceiving "forms," such as those of melody. Some twenty years later came the Berlin and the Leipzig schools, which were not so much concerned with "forms" (in the ordinary sense of this word) as with "wholes." Another and more inward difference is that the older school lived in what has been called the "classical" frame of mind. The later comers were, instead, of the "romantic" type.

For my own part, here as elsewhere in science, I have but scant sympathy with the "romantic" spirit (despite the attractiveness of the name itself). The writings in this spirit seem to me overgrown with equivocal verbiage; or with mysticism; or with both together, whereas the classically minded older school—as instanced by Meinong, Stumpf, Husserl, Benussi, Witasek, and Bühler—displays the utmost clarity and sobriety. Such works I have always regarded as very precious. In fact, they set forth much of what has above been called the educing of relations. If, actually, I borrowed little from them, this has been only because I reached most of them too late. In the meantime, I had gone back to the writings of the Scholastics and there had found the bulk of the Austrian work already forestalled; often, indeed, overtopped. And by the time that I had followed out the lines of thought and research to which this ancient literature had stimulated me, there was little left to get even from Meinong. In closing here this sketch of my more intimate contacts with contemporary psychology, I do not, of course, imply that the latter has not also had other movements of equal or even greater importance (e.g., psychoanalysis); only with these my work happens to have been less directly connected. Consideration of them will be reserved for another occasion.

Finally, although in the preceding pages my work is represented as but little indebted to modern writers, this must be taken to refer only to fundamental matters. For *details,* it is to the authors of the present day that we must go every time; scarcely a day passes but that one meets with some or other modern experiment which has elicited facts of lively interest though in a narrow way.

And thus at last I am brought back to Wundt with his epoch-making introduction of the experimental method. To him and to Galton I certainly owe far more than to anyone else; so that I cannot end better than by reiterating my grateful acknowledgments to these two great inspirers of modern mental science.

WILLIAM STERN*

There is no intellectual undertaking less suited to casual treatment at some outside instigation than the task of writing an autobiography. To stop thus and contemplate one's self is not feasible simply upon command; it requires some inner motivation—be it through old age, that brings with it a desire to say a conclusive farewell to one's own becoming, or be it through some epoch in one's life that invites one to rest, to look before and after.

I am actuated at present by the second motive. Participation in this collection of self-portraits seemed impossible to me as long as the systematic presentation of personalistic philosophy had not been completed. But now that a point of rest—tentative at least—has arrived with the publication, in 1924, of the third volume, a desire arose in me to view my life in retrospect. The way in which my personalistic convictions have crystalized out of an originally different point of view, and have asserted themselves more and more; the involved relations—full of tension and complexities—obtaining between my two great fields of interest, philosophy and psychology; finally the evolutionary process which the system of personalism itself underwent in my own thinking—all these matters I sought to recall and present.

A fully detailed manuscript has—for want of space—been condensed into the following report, which contains three sections dealing with the chronology of my work from the beginning of my university career to the present day, and one, the last section, giving a systematic survey of the fundamental personalistic tenets and their bearing on the special sciences.[1]

The theme of this self-portrait is my *philosophical* development. My strictly scientific activities in the field of psychology are to be touched upon only in so far as they are somehow related to the train of philosophical thoughts.[2]

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[1] The last section was originally conceived as an epilogue to the complete presentation of the system, but at that time (1924) no conclusion was written.

[2] But the psychologist cannot efface himself entirely, for the reason that the *first phases* of philosophical development are treated, for the sake of the juvenile psychology which they contain, more fully than their objective significance would merit.

The outer frame of my professional fate may be sketched with a few words. The scenes of my life were laid in just three cities: for twenty-five years at Berlin (where I was born, as the son of a merchant, on the twenty-ninth of April, 1871, attended the "Köllnisches Gymnasium" until 1888, then the University, from which I graduated in 1892); nineteen years at Breslau (where I became Instructor in 1897, Associate Professor in 1907); and since 1916 at Hamburg (where I first held a professorship at the Colonial Institute and Lecture Fund, and in 1919 became full Professor at the newly-founded University).

BEFORE HABILITATION (1888-1897)

In the fall of 1888, a youth of seventeen and a half years, I entered the University of Berlin with the intention of studying philosophy and philology; for I was planning to follow the example of my maternal grandfather, who had been a respected academician and scholar, and become an educator. But I realized very quickly that the petty pursuits of philology could not thrill me; within a little while I was devoting myself entirely to philosophy and psychology.

The picture which the German universities of that day (especially Berlin) offered with regard to philosophy was very uninspiring; to say it briefly, they exhibited no courage of philosophical conviction. The collapse of speculative philosophy after the death of Hegel had had a paralyzing effect, the triumphal procession of natural science a downright hypnotic one. The professional representatives of philosophy were for the most part content[3] to evade the situation by turning their gaze backwards: either to the history of philosophy in general, or particularly "back to Kant." Some saw the essence of philosophy in its history, others in epistemology. Some thought to attain the objective spirit of history through the utmost suppression of their own point of view, others found in epistemology essentially a justification for not having any opinion on ultimate, i.e., metaphysical questions. The conception as well as the word "metaphysics" was in disgrace, and was regarded as a remnant of a transcended age. If, in spite of this, some attempt was made at an individual philosophical outlook upon life, it was crippled from the outset by the crushing influence of natural science. The mechanistic categories of the scientific philosophy were regarded as self-evident and unshakable; the only possibility that remained open was to interpret them and supplement them from the standpoint of the rational sciences, as well as to deduce from them some general consequences for cosmology—never to take an independent attitude toward them and affect them from some higher peak.

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[3] Apart from a few Hegelians, who—like Adolf Lasson—rose up into our world from some perfectly alien one.

Incidentally, such groping efforts at philosophical individualisms (Lotze, Wundt, Paulsen) found little response in university circles. Still less respect was shown to systems of philosophy in which appeared already a certain note of protest against the mechanistic conception of the world which natural science had introduced: of all Fechner's writings, it was just his book of world-conception, *Die Tagesansicht,* that was least known; and the only thinker who really knew how to penetrate into the depths of metaphysical problems, Ed. v. Hartmann, was entirely unknown to the academic world. His name was hardly ever mentioned. Even to me it did not become familiar until after my student years.

Under such conditions, the mental and educational intellectual influences of philosophy instructors upon their students were rather pathetic. Among the students, the hatred of metaphysics took the form of simple hatred toward all philosophy; that anyone who did not for reasons of academic credit have to attend philosophy lectures should do so from choice was almost unthinkable. Live spirits who hankered for a world-view sought to find it not through their professors, but through the cultural tendencies of the time, which all had their origin in natural science: naturalistic art, Marxian social theory and the materialistic theory of the world and of life.

An immediate view of university life of that period is to be found in my diary, which I kept conscientiously during the first semesters. It contains among other things a lengthy treatise, which undertakes "a general and careful critique of the new tendencies in art, politics, religion, and philosophy,[4] among which I think I have discovered an intimate connection." The common element I thought to find in two characteristics: in the paucity of historic approach and in the overestimation of scientific standpoints, which were uncritically applied to aesthetic, rationalistic, and ethical problems.

At the same time the diary indicates with fair exactness the time when I began to be aware of the philosopher within me. It was during the second semester; in greatest excitement I scribble page upon page of philosophical reflections, and refer to a few weeks of quiet, joyous, self-sufficient philosophizing as "the happiest time of my life."

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[4] Here is meant not academic philosophy, but popular naturalism.

Of course I was rather lonely in my philosophical interest; I did not find a single fellow-student who might have been my philosophical friend and intellectual companion, nor any instructor toward whom I might have borne the feelings of a disciple. But with gratitude I here think of Paulsen, whose lectures and seminars were for me the gateway to philosophy, and gave me practice in philosophical discussion. Yet, more than this first incentive, he was not able to give me; once awakened to philosophy, I now went my own ways, and tried to progress autodidactically through much reading and notation of my own ideas. Only much later I realized what a loss it had been that as a student I had formed no closer relationship with the hard but significant personality of Dilthey.

As regards content, this philosophizing of an eighteen-year-old is, of course, very vague and has little depth. The influence of Paulsen is noticeable in an empirical tendency and a predilection for Schopenhauerian voluntarism; the problem of knowledge is regarded psychologically. A mechanics of feeling is taken as the basis of character. The importance of Kant seemed to me to lie in his repudiation of metaphysics; and a tendency to speculation, which I discovered in my own make-up even then, I regarded as a weakness. "I am sure that at the university I would have been drawn into the wake of the Hegelians, had not Paulsen trained me in his own sober, utterly unmetaphysical ways, which lack indeed all flight of genius, but are the guide and check for the beginner." It was a decade before I was candid enough to grant this speculative tendency its own rights. But it is noteworthy that even at that time I held the opinion that for the explanation of organic processess a purely mechanistic theory of motion was not adequate.

During the third and fourth semesters I became acquainted with psychology, and that in two very diverse ways. In the lectures of Lazarus I welcomed the fact that my own thinking, which (in the diary) had long been dealing intensively though unsystematically with psychological problems, at last received some orderly formulation, through the simple categories of a somewhat modified Herbartian psychology. But much deeper was the impression which the lectures and exercises in experimental psychology, given by the young Hermann Ebbinghaus, made upon my mind.

There were two things that commanded my profound interest. In the first place, Ebbinghaus' point of view appealed to my love for the empirical, which is one of the main lines of my personality, quite as much as the love for speculation; I had a strong desire for exact detailed work and direct contact with the concreteness of fact. My speculation and meditation required to be counterbalanced by objective reality. This was given by experimentation, the physiological explanation of psychic events, the measuring of sensory thresholds, and of reaction-times.

The other attraction, however, was again of philosophical character. The demand which I myself had always deemed proper, that the natural sciences be given their due weight and application without being allowed to mechanize our whole view of reality—that demand seemed here to be met. For here the exactness of natural science seemed to have been carried over to realms of the psychical, without apparently disturbing the autonomy of those phenomena; since it was held that, by the fictional hypothesis of psychophysical parallelism, which since Fechner's day had been taken for granted as the basis of all these speculations, the psychical realm had forever been safeguarded against any encroachment on the part of physical science. In place of such encroachments, a functional relationship was supposed to hold sway (Fechner's psychophysical law), a grand cosmic law which seemed exalted above all previously discovered laws in that it governed not merely the relations obtained among psychical elements, but appeared to control the entire connection of the psychical as such with the physical as such.

Here, then, both the cosmological and the empirical interest found encouragement; well do I remember the excitement with which I devoured Fechner's psychophysics, and at the same time became absorbed in the subtleties of psychological experimentation. Ebbinghaus' excellent teaching, his spontaneous, plastic, humorously tinged delivery, the picturesqueness of his examples all helped to fire with enthusiasm the little group (at that time) of his students for psychology as he conceived it.

He himself, it must be admitted, was much more an empiricist than a philosopher; he was less inclined to go with Fechner into the ultimate consequences of parallelism and panpsychism than to remain upon the solid ground of fact, to dig here industriously, and seek to bring all accepted hypotheses into relation with demonstrated or at least demonstrable facts. I learned a great deal from him, not only the technique of precise experimentation, but also that particular habit of psychological thinking and interpreting that tends to bring the psychical into close relation to all physiological and biological aspects of the individual. I emphasize this especially, as I was later forced to differ from him on so many of the most salient points, in regard to philosophical premises, as well as in the psychological interpretation of many special matters, especially the higher mental processes.

At nineteen I saw the pathway of my life that I was to tread (and have trodden to the present day) before me in perfect clarity. On the last pages of my diary one may read the following decision, stated in youthful heroics: "It is over, now. All bridges are broken, there is no retreat. In philosophy I must find my salvation, or perish. One consolation there is, namely that a philosophical discipline is open to me as my special field, psychology. . . ."

Although the rest of my student years were devoted almost exclusively to psychology and allied exact sciences, I have never, like so many other experimental psychologists, become "scientificated." The connection with philosophy and the humanities was always evident to me. It must be confessed that, at the time, I thought to solve the antithesis of the natural and the rational sciences somewhat too easily, in a paper that remained unpublished; psychology was to be the mediator; for (being the exact science of spiritual nature) psychology was one of the natural sciences, yet it was the foundation and premise of all intellectual sciences, which really were nothing but "applied psychology" (the term is therefore as old as this). Incidentally, this psychologistic phase of my thought was only of short duration.

Here one may already see a trait which was to appear again and again in later attitudes—that my mind is averse to the radicalism of narrow-mindedness, but that it is ever my ambition to overcome the partial truths and partial errors of opposed views by a process of synthesis. But at that time I lacked utterly the *radicalism of synthesis;* I had not yet discovered that one can effect a new world-outlook only from a much higher standpoint than either of the previous views, but hoped to find a solution on the same level as the paradoxes themselves.

The connection of my psychological researches with humanistic problems was supported by my first independent investigation, which I carried on, besides my experimental work, during my last student-years, and finally made the subject of my dissertation. Shortly after my graduation it appeared in book form with the title, *Die Analogie im volkstümlichen Denken.*[5]

This essay, which was entirely psychological in its approach, I regarded as a necessary prologue to logic, since analogy seemed to me the most elementary process of thinking and the foundation of all higher types of reasoning. Thus the book was supposed to be a contribution to the descriptive methodology of naïve reasoning. A very wide interpretation of the word "analogy" enabled me to select my examples of the origin and application of analogy (in which I included argument from analogy) from the most divers sources: myth, language, child psychology, even animal psychology—and to dissect these critically.

As I now look back upon it, the book seems to me more important for the intuitive grasp upon psychical affairs, which was evinced in this work for the first time, than for the psychological theories contained in it, which in the main were still content with the simple scheme of mechanical association. In this respect I was still very dependent upon my two teachers, Lazarus and Ebbinghaus, who, however striking their differences in other directions may have been, must both be numbered among the association psychologists.

After my doctoral examinations, I devoted myself to private researches, especially experimental ones, in various institutes: with Ebbinghaus, then—after he went to Breslau in 1894—under his successor, Stumpf, furthermore with Arthur König, the physiologist. Here originated a series of researches which were all devoted to a very general theme, the *Apperception of Change,* and which were concluded only in my habilitation thesis.

In the year 1896 my mother died; I had been an only child, and had always lived with her, and contributed to her support through giving private lessons, since the death of my father in 1890; now I was alone. At the same time I realized that my prospects of an academic position at Berlin were hopeless for the time being. Just then I received unexpectedly a suggestion from Ebbinghaus to try for habilitation at Breslau. I concluded the complete presentation of the "Psychologie der Veränderungsauffassung," and, on the strength of this essay, received the position of Instructor at Breslau in the Summer of 1897.

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[5] Die Analogie in volkstümlichen Denken. Berlin: Philos.-histor. Verlag, 1893. Pp. 162.

In 1898 appeared the book of the same name.[6]

The six years' labor which I devoted to the problem of change must be viewed as a whole, because it involves a decisive metamorphosis of my evolving intellectual outlook.

The issue was raised by a psychophysical proposition: I wanted to discover the "sensitivity," not as Fechner and his successors, for two barely distinguishable constant stimuli, but for the continuous change of one stimulus into another. At first I conceived the problem in purely sensationist terms, sought to determine thresholds experimentally, raised the question of the possible existence of "transition-feelings," etc. Soon, however, the sphere of my inquiry widened in the direction of descriptive and humanistic problems.

The descriptive method—first employed in the monograph of 1894, *Die Wahrnehmung von Bewegungen vermittelst der Auges*[7]—quickly gained independent importance.

I observed—and had already taken many notes on the subject—that there are a number of general conditions, which, quite apart from the sensory field, are *conditiones sine qua non* for change as such: substratum and varying properties, in the latter temporal succession, successive variation, constancy; that, furthermore, consciousness of such characteristics as persistence, tempo, degree of change, have to be grasped descriptively; thus, under one heading should appear transition, process, becoming; under another, quantitative, qualitative, local variation; under a third, alterations of higher and lower order.

All that was published on this topic in the habilitation-thesis was the program. I regret now that the fairly extensive manuscripts on the subject never reached their publication; for, so far as I know, they represent one of the earliest attempts at what is now called "phenomenological description," and in spite of their incompleteness, might have commanded some degree of attention from the phenomenological work of later years.

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[6] Psychologie der Veränderungsauffassung. Breslau: Preuss u. Jünger, 1898. Pp. 264. 2nd ed., 1906.

[7] Die Wahrnehmung von Bewegungen vermittelst des Auges. *Zsch. f. Psychol.*, 1894, 7, 321-386. Also Hamburg and Leipzig: Leopold Voss, 1894. Pp. 68.

Likewise unpublished remained the psychogenetic studies in the conception of change. These began with the pre-scientific thought from whose aporia the scientific treatment of this category first arose. "Alteration" is just one of those conceptions "whose first philosophical formulation is a product of a long preceding development." The philosophical history of the concepts was traced by extensive study of source materials, which for the time being took in the Greek thinkers and Leibniz.

The insights here attained have become important for my own philosophical convictions through the fact that they revealed to me the extreme artificiality, indeed the paucity, of a mechanistic view of nature, and the unreality of any abstract theory of being. The fullness and diversity of types of change had impressed me so definitely in the course of my phenomenological observations, that their total reduction to the one monotonous form of change of location could not help seeming to me like a dreadful impoverization of the world-picture. And, likewise, the inherent unity of any change whatever with the substratum in which it occurs had become so evident to me, that a conception of substance which was supposed to be characterized by stark immutability became for me utterly untenable. Without my having any possible inkling of it, the new qualitative and vital substance-concept of the Person was here already in the making.

Yet the conclusion of the work, which appeared in book-form in 1898, restricted itself in the main to an experimental and psychologically explanatory treatment of the apperception of change. If one compares it with the sensationalist and element-psychological point of departure of those researches (cf. p. 8), one will easily see what a great distance I had already traversed in the direction of a Gestalt-psychological and personalistic re-establishment of psychology. And all real results that I attained were due to this change of approach.

Even the new expression, *"apperception* of change," instead of "change-perception," is to be understood in this sense. Change has taken on the character of a categorical *Gestalt,* the experiencing of which is to be explained. To this Gestalt, temporal continuity belongs intrinsically, and just for this reason the original experience of change can consist neither of a momentary change-sensation (which as an "index of change" would have at best secondary meaning), nor of an abstract comparison of two successive but essentially constant momentary stimuli; but must be given in some entirely different kind of awareness. "There are not only momentary, but also *temporally extended and yet single* acts of perception, and in such we must seek the source of the full perceptual impression experience of change."

I had realized immediately that this train of thought had a general significance beyond the mere apperception of change and combined itself well with beginnings of ideas that had been uttered by others, into a thesis of greater scope, concerning which I published an original paper entitled "Psychische Präsenzzeit."[8] "The psychological event covering a certain period of time may under some conditions constitute an integral unit act of consciousness, despite the non-contemporary nature of its parts. The period of time covered by such a psychical act I called a present-time." The psychological "now" is not, like the mathematical "now," a differential, but is of finite though perhaps very small magnitude, contains within itself duration and organization. All attempts to cram such an experience, as, for instance, a rhythm, into a psychical "point" of time are absurd.

Even where the book on change treats of the quantitative laws (the "finesse") of change-excitation, one can see the mutation of the point of view. "Excitability,"—no longer "sensitivity"—is the object of research, i.e., the way in which the *activity* of man responds to change-stimuli. Definite leave is taken from a passivistic psychology that recognizes only the coming and going of sensations and ideas. And more than that: the laws of change-excitability which I had discovered (concerning the dependence of reactions on the rate of change and on the subjective rhythms of the observer) apply as well to awareness-reactions and judgment as to motor reactions; the fact being that they are valid—as today I would express it—for the *psychophysically neutral activity of the individual.*

The hope which I attached to the book on change and the article on "present-time," that their problems, once formulated, would be tackled by the contemporary psychological profession and treated in detail, was not to be realized. Today I comprehend that the book was doomed to failure because it was not suited to the time. The excursion into Gestalt psychology and personalism was something of an alien element in the scope of a psychophysical inquiry. And the fact that I myself was not at all clear concerning the import and philosophical foundation of the new point of view could only render it more unacceptable to others. Not less than two decades later the book might have counted on a cordial reception—had it not, by that time, been long lost to the profession.

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[8] Psychische Präzenzzeit. *Zsch. f. Psychol.*, 1897, **13**, 325-349.

The Period of My Instructorship

In the autumn of 1897 began my teaching activities at Breslau, which covered psychology, philosophy, and pedagogy.

In the spring of 1899 I was married.

My intellectual labors now begin to divide themselves between psychology and philosophy.

My philosophical inclination grew more and more into a *furor metaphysicus,* which, however, burned only internally. The desire for conviction in "ultimate things," for the construction of a new world-aspect had to find its own way perfectly unaided. I knew that with these ambitions I should have to "withdraw without hatred from all the world," for years and perhaps decades, and was ready to assume this part; for the solution of that problem required insensibility to the lack of understanding and the passive resistance of the rest of the scientific world. For a long while I suffered from a veritable repression when it came to talking to anyone else about philosophical matters, because I knew so well that for my way of seeing things I could not expect to find any sympathy; conversation would only result in talking past each other. And thus the philosophical isolation in which I found myself at Breslau, due to the absence of other young instructors and the cool reserve of the ordained professors in my field, was almost welcome.

Yet I was nothing less than a world-removed hermit and dreamer; I have already mentioned that other trend of my nature, which is all toward direct action, real objectivity, and concrete detail work. My special science of psychology offered me an opportunity, which I gladly seized upon, to cultivate these interests. In the first place, it commands my enthusiasm for its own sake; in the second place, it gives me a proof—indispensable to every research worker—that my activity was contributive to the scientific interests of the rest of the world. Besides, it was the stockade, so to speak, behind which the other, metaphysical structure could unfold and rear itself, invisible and unmolested.

Just as important for the structure of my personality as this dualism, however, is the tendency to overcome it. It took me decades, though, to bring this synthetic ambition to full and clear consciousness. And during these decades, phases of closer or remoter relations between the two realms alternate with one another. My early psychologistic illusion, that psychology should be able, just by itself, to solve the problem of a world-perspective had long since been dispelled; but, as a reaction against it, I still felt an occasional desire to confine psychology entirely within the bounds of a special science and to pursue philosophical problems entirely without its support. But that was more a displacement (*Verdrängung*) than a real isolation of the two realms against each other; constantly there are invasions of the one by the other, again and again—usually without my taking any real account of them, but sometimes also in a fully conscious way.

Perhaps my special psychological researches sometimes suffered thereby, suffered a certain ill-humor. I myself was not able to appreciate the full importance of the departures that were starting from the dawning personalistic point of view; and my colleagues could not but take exception to unfamiliar lines of thought which as yet could claim no definite foundations. The first of my conclusions and views to gain recognition and become influential were those which summarized and elaborated current tendencies of contemporary psychology, not those which indicated an imminent reorientation of psychological foundations. I myself was not free enough to escape from the bonds of my scientific inheritance to such a degree as the reorientation would impose; in fact, at times one could have detected actually an approach to the point of view of element-psychology, from which, inwardly I was by that time far removed.

This vacillating state of mind is mirrored in three lectures, entitled "Century Retrospections," given in the winter of 1899-1900.

Here the relationship between philosophy and psychology is summed up in the formula "march separately, strike together," although the sovereignty of philosophy is upheld as insistently as psychologism is denied. As regards the evolution of psychology itself, the "scientification" of the subject is duly respected, but the alleged right to deduce from this a mechanization of spiritual life is not admitted. In the contemporary research, a distinction is drawn between "subjectless psychology" (which would turn the fertile working concept of analysis mistakenly into a principle of being) and a "subject-psychology" which enlists my sympathies.

But even my scientific activities are determined by philosophical motives; this may be seen in the very choice of problems, whose scope far exceeds that of commonly accepted fields of research. To the problem of change, in itself a bold departure, is added now the problem of *individuality.* In 1900 my book *Über Psychologie der individuellen Differenzen*[9] appeared, which—somewhat luridly, yet with correct apprehension—in its preface hails the topic of individuality as "the problem of the twentieth century." In essence it was a programmatic work, seeking to indicate the outlines of a new discipline, "differential psychology."

Scientific psychology had so far been mainly a generalizing science, and had regarded individual differences, which occasionally appeared in the course of experiments, more as a regrettable hindrance to its own generalizing tendencies than as a genuine problem. Thus the natural interest in individual variations was left entirely to unscientific treatments (e.g., phrenology, graphology, etc.). This condition I wanted to rectify through differential psychology; I undertook to give the psychological differences between one human being and another the status of an independent theoretical problem, to be handled with appropriate scientific methods. Thus the concepts of type, norm, and aberration come under discussion, as well as the internal and external conditions of differentiation, and finally the nature and utility of the symptoms which indicate externally the peculiarities of a mind.

It must be admitted that even then I saw the limitations of this method. For real "individuality," the understanding of which I had made my goal, cannot be reached through channels of differential psychology. For this there are two reasons: one, that differential psychology *dissects* the unity of spiritual life; the other, that this science, just like general psychology though to a lesser degree, *generalizes.* For the concept of a "type" is itself a general functional rule for a group of human beings; the relegation of an individual to a type or to several types can never do justice to the ineffable particularity of his individuality. But to this limited scientific point of view we may oppose two others which directly reveal the personal unity: art and metaphysics. "For these are ever seeking to give us the particularity of the individual, to let us apprehend that behind that wealth of most various expressions—the sole interest of sober, ordering science—there lies a synthesizing higher unity. Not a complex of differential forms of psychical phenomena, but a genuine individuality, something indivisibly singular, a personality."

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[9] Über Psychologie der individuellen Differenzen (Ideen zu einer differentiellen Psychologie). Leipzig: Barth, 1900, Pp. 146.

Thus personalism is already recognized as a metaphysics. But as yet a real unification would be premature. Differential psychology, like its elder sister, general psychology, is still supposed "to carry on its researches with the least possible reference to metaphysical problems."

The principal content of the book is scientific in character and therefore does not belong into the frame of this self-portrait.

With this book I invoked the first response from my colleagues for my psychological ambitions. Within a few years it was out of print, but it has never been reprinted; in its stead there appeared, in 1911, another, entirely new book.[10]

Further specialized researches in the psychological field may also be mentioned here in merest catchwords; only a few points must be specially mentioned, which refer to philosophy.

Certain experiments on reliability of memory, originally conceived as theoretical studies in the differential psychology of memory, presently appeared to be contributions to *applied psychology* of testimony (1901).[11] The significance of these errors of testimony for justice, pedagogy, history, and psychiatry naturally commanded general interest outside the psychological profession, and soon it appeared desirable to create a meeting-place for the various workers, psychologists and others, in this field, namely the "Beiträge zur Psychologie der Aussage" (2 vols. 1903-1906). The most detailed study contained therein is that concerning my experiments with school children, using pictorial material: "Die Aussage als geistige Leistung und als Verhörsprodukt."[11a]

It contains—besides the results in psychology of testimony—a *genetic-psychological hypothesis*: for, from the way in which children make the content of pictures their own, I tried to deduce a principle of succession for the development of the categorial functions, which should then be valid for other types of intellectual activity as well; a "substance" level is followed by an "action" level, above this there is a level of "relation" and finally "property."

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[10] Die differentielle Psychologie in ihren methodischen Grundlagen. Leipzig, 1911 3rd ed., 1921. Pp. 545.

[11] Zur Psychologie der Aussage. *Zsch. f. d. ges. Strafrechtswiss.*, 1902. **22.** Also Berlin: Guttentag, 1902. Pp. 56.

[11a] *Beiträge zur Psychologie der Aussage*, 1904, **1**, 267-415.

The predilection I had at that time for projects caused me to preface the "Contributions" of 1903 by an essay on "Applied Psychology"[12]; it discusses the concept, characteristics, and specific methods of a science which at the time could show only diffident beginnings. Among the practical problems which I there point out or presage are many which later actually became provinces of central interest of applied psychology; moreover, the word and the concept of "psychotechnique" is already to be found in this article (proving that it was not, as many believe, introduced by Münsterberg in 1914).

But a high opinion of the services which psychology could offer to civilization must not be interpreted as "psychologism." This is definitely protested here: psychology is essentially an auxiliary, not a foundational science for the study and practice of human civilization. The basis of this attitude is a philosophic argument of peculiarly dualistic character: psychology, with its indifferent, naturalistic interest is supposed to be necessarily inadequate wherever "spiritual existence as an individual whole, i.e., in the form of personality," is under consideration. Such a "de-personalizing" of psychology (which appears almost like a regression from the conception of 1900) is the more surprising because in my philosophical outlook the personalistic strain had become very pronounced.

In this dualistic turn of thought, the influence of the Southwest-German group of philosophers, Windelband, Rickert, and Münsterberg, is unmistakable. Like them, I inclined at this time toward the view that psychology was purely an analytic and mechanizing science, whereas personalistic aspects of mind belonged essentially to the realm of values, and to the sciences related to it, moral, historical, and practical. This was indeed nothing but a renewal and extension of the Kantian two-world system; such a dualism, of course, could be no more than a provisional standpoint for my essentially unity-seeking mind.

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[12] Angewandte Psychologie. *Beiträge zur Psychologie der Aussage,* 1903, 1, 4-45.

At the same time, *child psychology* entered my field of vision from various directions. The experiments in testimony performed with children have already been mentioned. Another study of school children, conducted by the questionnaire method (and therefore meeting with some disapproval from the profession), dealt with *taste and distate for curricular activities;*[13] it sought to establish the childish trends of interest, the motivation of their likes and dislikes, the distribution of positive and negative judgments over the various subjects, the dependence of childish attitudes upon age, sex, environment, personality of the teacher, etc.

At the same time, in the quiet retreat of our own house, a very different sort of psychological material was gradually produced; our *diaries* recording the early development of our own children. They were conducted by my wife, who frequently discussed and conferred with me upon the subject.

In unconstrained fashion, and from various angles, these books report the development of three essentially very different children from birth well up into the school years. They were intended to form the basis of six monographs; unfortunately other urgent duties have prevented the realization of this plan. What bits of this material were utilized later either by my wife and me[14] [15] or by me alone[16] form only a small fraction of the extensive reports we had collected.

But for my own real development these studies of my children have had a further significance. Here I observed *concrete* spiritual life and was thereby safeguarded against those false schematizations and abstractions which we meet all too often under the name of psychology. Here I became aware of the fundamental personalistic fact of *unitas multiplex;* the wealth of phenomena concommitantly or successively observable arrayed themselves in a unified life-line of the developing individual, and received their significance directly from this. Here I discovered the fundamental forms of personal causality: the convergence of the stirring character-traits in the developing child, with the totality of environmental influences. In short, here I gained important conceptual foundations for the dawning philosophical theory.

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[13] Über Beliebtheit und Unbeliebtheit der Schulfächer. Eine statistische Untersuchung. *Zsch. f. päd. Psychol.,* 1905, 7, 267.

[14] With Clara Stern. Monographien über die seelische Entwicklung des Kindes. I. Die Kindersprache. Eine psychologische und sprachtheoretische Untersuchung. Leipzig: Barth, 1907. 3rd enlarged ed., 1922. Pp. 434.

[15] With Clara Stern. Monographien über die seelische Entwicklung des Kindes. II. Erinnerung, Aussage und Lüge in der ersten Kindheit. Leipzig: Barth, 1908. 3rd enlarged ed., 1922. Pp. 160.

[16] Psychologie der frühen Kindheit bis zum sechsten Lebensjahre. (Mit Benutzung ungedruckter Tagebücher von Clara Stern.) Leipzig: Quelle u. Meyer, 1914. Pp. 472. 4th ed., 1926.

The problem of convergence dominates another research, in which another method, namely, that of literary analysis, is employed—my study of the deaf-and-blind subject, Helen Keller.[17]

In the life of this rare woman, I saw a psychological experiment of Nature in a grand style, and tried to represent on the basis of her autobiography the course and the factors of her mental history, the construction of her world from tactual sensations, the unusual, yet in itself remarkably well-ordered process of acquiring language through the tactual finger-alphabet. Four years later I had an opportunity to meet Helen Keller personally, and to corroborate and complete my previous impressions of her.[18]

At this point we must go back a few years, in order to trace *the first phase of my development as a philosophical system-builder.*

My interest in problems of empirical world-perspective had perhaps been crowded out occasionally by psychological labors; but it had never really been asleep. It received a new spark from my lectures on contemporary philosophy; and, when in 1900 the completion of my book on individual differences left me at leisure to undertake a new work, I was not for a moment in doubt as to what I should do next.

The time from the summer of 1900 to 1901 saw the real birth of Personalism.

In the Spring and Summer vacations, in the calm retreat of a little Silesian bathing-resort, I cleared my soul of all the ideas which had been hidden in it in a vague and confused form by prolonged philosophic thought. Notebook on notebook filled rapidly that year with unsystematic scribblings, from which gradually emerged clearer and more systematic formulations.

The fundamental motive of all my philosophizing—then as now—was ever *the desire for concrete unity.* I felt equally dissatisfied with either of the two tendencies which dominated the intellectual life of those days: the *atomization* of the world, life, and civilization, on the one hand, and the postulation of an *abstract* unity, an all-devouring mathematical-mechanical causal law, on the other. In the former case, the patch-work heterogeneity seemed to destroy all unity, in the latter a fallacy of over-simplification deprived reality of all its fullness; science and life were either torn asunder or emptied of their content.

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[17] See footnote 7.

[18] Helen Keller, Persönliche Eindrücke. *Zsch. f. angew. Psychol.*, 1910, **3**, 321-333.

That it was essential to escape from this noxious atmosphere, I had realized long before I myself saw any way out of it. The need of philosophical orientation appeared to me not merely as my subjective desire, but as a crying need of the time. Incidentally I was not afraid to use, occasionally, the word "metaphysic" for the object of this desire: "the metaphysical problem is concerned not with the causes of reality, but with its meaning."

Thus it was the creation of such a metaphysical outlook that I regarded as my proper task. I suspected its enormous difficulty; I realized that it meant giving many years of my life to a work to which I might not be equal in the end, and that for decades, perhaps for ever, I was making myself an outsider instead of pursuing the gradual increase of existing knowledge with little steps that every one could follow. But the thrill of the task was greater than any doubt, and with the courage of one who is not yet thirty, I took the plunge.

Now, was this work determined through some definite tendency of the philosophy of the time? Can I claim to be a member of some school, a continuer in some definitely chartered way? This question I must answer in the negative. The professional philosophy of the day, at least so far as I had come in contact with it, could offer me no guidance in my search.

This must not be misunderstood. Of course I owe a great deal to the philosophers of that time, just as to the thinkers of earlier epochs. Wundt had emphasized the activity, Fechner the hierachical arrangement, Hartmann the teleology, Dilthey the concreteness, of Personality. The contrast between person and thing had found expression in Kant's famous dictum of the dignity that should belong to man alone, and the price that was proper to everything else; and it met with some analogous thoughts from the Southwest-German school of philosophers (Windelband, Rickert, Münsterberg) with their distinction between idiography and nomothetics. But in the system of Personalism all these ideas became mere dependent items, which—just because of their incorporation in a new intellectual totality—received a fundamentally new character. The relation of my philosophy with that of Aristotle only came to my notice in the course of elaborating the system.

But an immediate influence— as I see it now, in retrospect—was that of the exact sciences and of certain cultural tendencies of the time. Inorganic and organic sciences presented a picture such as I had met with in psychology: on the one hand, an attempt at the utter mechanization of their material, on the other, a timid protest against this ambition, or an unconscious dealing in illegitimate scientific categories that belonged properly to anti-mechanistic or hyper-mechanistic realms. Likewise in public life I found the contrast which now-a-days is popularly designated as the contrast of civilization and "Kultur"; men had become more and more enslaved by the mechanical contrivances of which they were so proud: by the productions of technology and economy, by their accumulated unorganized knowledge and senseless hankering for power—and the few voices that cried against this madness were as yet almost completely drowned out by the roar of the market-place.

Now the real philosophical task with which this state of affairs confronted me was two-fold: in the first place, a *synoptic view* of all the apparently separate, scattered, and dissenting antitheses under one great general principle: in the second, the opposition of teleology and mechanism, unit and aggregate ("person" and "thing"), which thus became for me the central motive of the philosophical conflicts of the epoch. Furthermore an attempt to solve this conflict of principles through some system of thought that should do justice to both these polar extremes, though indeed it would grant to one of them definite superiority. I found it as impossible to maintain a one-sided point of view, which should simply ignore the other, as to acquiesce to an eternal dualism such as had been repeatedly attempted from Kant to Münsterberg. I had to find a dialectic solution, which would not be a compromise, but a genuine *radical synthesis* of teleology and mechanism: and this was the concept I undertook to develop critically, the concept of *personality*.

Limitations of space preclude an account of the way my memoranda of this year gradually gave rise to certain recognizable points of view and fundamental categories, how there appeared even certain special notions—that of "teleomechanics" for instance, and the "birth of species." During the winter of 1900-1901 I outlined the first drafts of the book, and determined on the main title *"Person-Sache,"* as well as on the designation "Personalismus" for my general outlook. Soon, however, I conceived the idea of letting a little work entitled *Vorfragen der Lebens- und Weltanschauung* precede the systematic *magnum opus* which I did not, as yet, feel quite prepared to tackle. During the Easter vacation in 1901 I wrote the prolegomena; for I was thoroughly ready to express all the ideas, needs, and desires that centered about the conception of a world-philosophy.

The little manuscript remained unpublished for a decade and a half. I was unwilling to publish it before the system itself had materialized; for "it seemed too cheap and easy to paint needs and longings upon the screen without having done anything one's self toward their gratification." Only in 1915 I published it under the title *Vorgedanken zur Weltanschauung,*[19] after two volumes of the systematic work had appeared. Apparently the *καἰρόσ* for this little essay was ruined by the long delay; it commanded little attention. All the same, it still seems to me very suitable for an initial orientation in philosophy; it may be regarded at once as a contemporary document of the peculiarly bifurcated condition of the intellectual outlook around the turn of the century, and as an expression of the youthful philosophical enthusiasm of one who stands on the verge of creating a system of his own.

Two attributes are normative for the concept, "world-view" (*Weltanschauung*): (*a*) Its objective-subjective character. It does not, like religion or metaphysics of the old type, claim absolute validity but is limited by the subject (individual, national, ethnic). This limitation, however, is not a shortcoming, but an advantage: "Whereas in all mere objectivity and universal validity the particular character of the subject is necessarily obliterated, the world-view reveals the world to the subject, without making the world consume the individual." (*b*) A world-view as synthesis of philosophy of life and theoretical attitude. It "undertakes to reduce the relationship between intellectual knowledge and valuation to a single formula."

A world-view is *philosophical* if it seeks to give both scientific theory and valuation a universal, conscious, and critical character, and to account for the respective rank of the two subordinate systems. Here the primacy of value is asserted: an autonomous, indifferent cognition is indeed conceivable; but as it has no bearing on the world of values, it makes no contribution to life, and degenerates to an "indifferent art of juggling with ideas, or a ghostly shadow-dance."

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[19] Vorgedanken zur Weltanschauung. (Niedergeschrieben im Jahre 1901.) Leipzig: Barth, 1915. Pp. 74.

From the subjective angle, a world-view means essentially the self-contemplation of a "Kultur" with reference to its own unconscious drives; now reflecting rather the dominant ones [world-view as "Kultur-formula" (*Kulturformel*)], now the struggling, incipient ones [world-view as "Kultur-prescript" (*Kulturparole*)]. Moreover, it is an expression of the essence of its originator; sometimes expression of his being, sometimes of his wish. But even this participation of the individual must not reduce a world-view to mere subjective illusion; the following premise is asserted not only for art, but for philosophy as well: "This precisely is the accomplishment of a genius, that he wrests from the world fundamental essences which are really contained in it, but which he alone is destined to behold; from now on they are conquered, as truths, in the name of humanity."

The second part, *Unsere Zeit und die Weltanschauung,* attempts to sketch a picture of intellectual conditions around the turn of the century, in simplified outlines. The absolutely dominant feature in philosophy, art, religion, and standard of life is the lack of any world-view. But to a sharper eye, a change is already apparent: in the cessation of self-satisfaction (pessimism), in certain artistic reform-movements, in ethical revaluations, in the attempts—feeble as yet, indeed—at a new philosophical idealism. But to translate the will to a new world-view into an act, requires a complete reorientation, not only of standpoints but also of fundamental categories, which must put intellectual differences to the test; thus the essay ends with a reference to the new ontological alternative, Person-Thing.

The first attempt at a *systematic* presentation of Personalism, in short aphorisms with elucidations, dates from 1901. These (unpublished) "50 theses" served as the foundation for a "metaphysical colloquium" which I conducted at my home in the summer of 1901, with a small number of students, and at which we disputed hotly till far into the night—a somewhat peculiar undertaking, in the setting of that anti-metaphysical period.

In these theses the fundamental category of the "person" is already fixed quite unequivocally, as a unique and self-sufficient *unitas multiplex,* whose activity as a purposive function is directed toward self-preservation and self-development. Beside it a "thing" is a mere pseudo-entity, resoluble into constituents, subject to mechanical laws. Through the fact that every person embodies a multiplicity (which in turn consists of persons), a hierarchy is generated, ranging from the atom to the total personality, of which the best-known stages are the intermediaries of cell, individual, genus. Thus Being gains a new dimension. "Persons exist not only one beside the other, but one above the other." This upward stratification makes it possible to relate teleology and mechanism and overcome the dualism of person and thing: for every person imposes upon its subordinate parts the self-preservative function, that serves its own ends, as the law of their functioning. Only through this deduction can natural laws be intelligible at all; at the same time, however, it contains the limitation of such law.

It may readily be seen that I was scientifically oriented at that time. This interest furthermore induced me to take up a special problem, which in itself, indeed, was quite big enough: the problem of the relation obtaining between *energy* and *life.* My preoccupation with certain other natural philosophers of the time (Ostwald's *Lectures,* E. v. Hartmann's *Weltanschauung der modernen Physik*) constrained me to test by some concrete instance whether the teleologico-personalistic attitude could be maintained in the face of the physical view of the universe, or might even prove the more appropriate. In this way I arrived at a new conception of law, *the "bio-energetic" law,* through which Fechner's psychophysical law is projected into another plane and considerably widened.

In Hartmann I saw an attempt to draw from organic physics a philosophical conclusion against which I revolted; the doctrine of *pessimism*: that all life is merely a short episode in an otherwise lifeless, purely physical world, was supposed to be demonstrable through physics itself. For, the increase of entropy (reduction of thermal differences) in the universe, which was postulated in the second law of dynamics, would have to lead—long, even, before the accomplishment of a complete equality of all intensities—to a condition of such low degrees of intensity that plasma, and therefore life, could no longer be produced. The triumph of a purely physical view over that of teleology could not have been more forcibly expressed: the world as "thing," i.e., as a battlefield for purely senseless energy-processes, appeared absolutely dominant, the existence of significant living wholes reduced to a shadowy interlude. Hence it was necessary to examine the scientific validity of this apparent proof. The result was negative.

Presently I imparted my findings to Hartmann by mail, and had the pleasure of starting a lengthy correspondence with him, though indeed neither convinced the other in the course of it. Through later personal meetings, too—whereby I was deeply impressed with the clarified personality of the lone thinker—quite naturally, we could not arrive at any convergence of viewpoints. But these exchanges of ideas did give me the impetus to work out my thoughts more and more carefully and formulate them precisely. This gave rise to the essay, *"Der zweite Hauptsatz der Energetik und das Lebensproblem,"* which appeared in 1903.[20]

The central idea of this detailed study is that vital processes do not depend on the absolute differences of intensity, which, according to the law of entropy, are decreasing steadily though asymptotically, but upon the *relations,* which can be preserved by reason of the peculiar self-directedness of living organisms. A living individual—viewed dynamically—is just such a structure, which keeps its own tensions in proper equilibrium with those of the environment. This conception is supported by the demonstration of many biological and cultural facts, which show no unambiguous correlation between absolute experience of energy and work (the passage is somewhat obscure to me because I cannot judge its exact scientific meaning—*Translator*) ; all "development" is at once an economy of the energy-output (*Spannungsgrössen*) necessary to the realization of the aims of life.

Now follows the *generalization* of this idea. If the relation of life to energy is not invariable, then the particular energy-pattern called "plasma" does not represent the ultimate limit, beyond which life is impossible. Of course, we must look beyond our narrow geocentric-biological horizon. Vitality, the purposive functioning of concrete unities is the original significance of the world as such, and can therefore not be bound to the chance existence of technical prerequisites. There is no physical law that precludes the notion that

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[20] Der zweite Hauptsatz der Energetik und das Lebensproblem. *Zsch. f. Phil. u. phil. Kritik,* 1903, **121**, **122**, 176-235.

Life is constantly producing these (extra-dynamical) "machinery conditions" which are required for the support of dynamical relations between the living entity and its environment. The optimistic view, that—despite a possible constant decrease of energy-expenditures in the world—life not only maintains itself, but evolves higher and higher forms, can indeed not be proved—for it is a faith. But, on the other hand, this faith cannot be demolished by any known laws of physics.

This essay had to be written in such a way that it might be comprehended without any knowledge of the (yet unpublished) personalistic system of thought. This perhaps explains its harshly dualistic tone; as though physical and biological causality belonged in two utterly disparate dimensions. The conjunction of the two in the idea of teleomechanism could simply not be expressed herein. Later some of the leading ideas of this work, especially the "biodynamic law," were incorporated in the teleomechanic sections of the main work.

At last, in the year 1916, the *first volume of the total work* could be published. It bears the title, intended for all the volumes and finally preserved as a main title, *Person und Sache,* and the special one, *Ableitung und Grundlegung.* That, furthermore, I denoted the whole by a subtitle, *System der philosophischen Weltanschauung,* has sometimes been construed as conceit. But any other expression would have been an internal falsehood; for I derived the right and the duty to such an intellectual construction from my conviction that I had found the decisive alternatives for the spiritual situation of the time in the two categories, person and thing, and the only possible solution of just this dualism in the philosophy of critical personalism. If this conviction was merely a subjective delusion, at least it had the consequence of inspiring the completion of the total work. For otherwise I would inevitably have been discouraged by the thorough indifference which, for the present, my volume encountered. As it was, however, I followed up this volume with two others, regardless of the indifference of the philosophical and scientific world, though indeed at fairly long intervals. And now it seems to have been worth while that I persisted. For there are many indications that the time of latency is nearing its end. I am no longer as solitary with my personalistic convictions as I was two decades ago.

From this point on the style of this self-portrait must take on another tone. The three volumes of the systematic work compose a unity, so that their description cannot follow a chronological, i.e., disrupted, pattern. Thus we shall dispense here with a detailed account of the contents of Volume 1 (referring the reader to section 4, p. 368).

III. 1906-1926 (AS PROFESSOR AT BRESLAU AND HAMBURG)

Since the record of my life from 1906 onward divides naturally into a chronological account and a systematic one, the former may be treated all the more briefly.

After the publication of the first volume of my philosophy I became completely absorbed, for a considerable length of time, in psychology. In that very year, 1906, the Institute of Applied Psychology was founded at Berlin, and—supported by private means—was put at the disposal of the German Psychological Society. In it was realized an older idea of mine for an organization, suggested by the increasing rapprochement between psychology and the realms of its application in science and common practice, namely, the creation of a center for psychological cooperation, summational research, and bibliography. The Institute was conducted first by me and my former pupil, and later collaborator, Otto Lipmann, afterwards, i.e., since 1906, by him alone. Just after its foundation we started the *Zeitschrift für angewandte Psychologie,* which has become the chief organ for this field of study. Later we added to this journal the *Beihefte* and the *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.*

The year 1907 brought me, after the departure of Ebbinghaus, my associate professorship at Breslau, and the task of conducting the University Seminary in psychology.

Meanwhile our diaries concerning our children had been worked over enough so that two monographs, prepared by my wife and me, could be published: in 1907 we issued *Die Kindersprache,*[21] which, besides a detailed account of speech-development in our own children, is also based on the entire known literature of the subject, and tries to exhaust the problem through the psychological and linguistic approaches. In 1908 it was followed by *Erinnerung, Aussage und Lüge in der ersten Kindheit;*[22] this book, too, is conceived at once psychographically and under the categories of comparative psychology.

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[21] See footnote 14.

[22] See footnote 15.

During the summer vacation of the same year, an academic anniversary celebration took me to America, where I received an honorary Doctor of Laws from Clark University. The few weeks of my study in America were rich in impressions and inspiration.

In 1911 appeared *Die differentielle Psychologie in ihren methodischen Grundlagen*.[23] This voluminous book took the place of the *Psychologie der individuellen Differenzen*, a methodology appeared in place of a prospectus. It owes its special character to the peculiar half-way position which differential psychology occupies, between "classical" psychology, on the one hand, and individual diagnosis and description, on the other. Whenever differential psychology seeks lawfulness in psychic variations, establishes connections of types, sets up determinations of variability and correlation, it is *generalizing*, though in a way distinct from that of traditional psychology (cf. supra p. 347) ; but, whenever it seeks to grasp the psychical make-up of an individual, his character, the degree of his intelligence, the total pattern of his personality, then it is *individualizing*, and is thereby approaching to the historical sciences. From this we obtain certain specialized methodological characteristics of differential psychology, certain categories of procedure, which are all treated of in detail and illustrated by concrete examples. For me, however, this gave rise to a peculiar conflict of which I was, perhaps, fully aware only in retrospect, namely, the conflict of an analytic point of view, that of element-psychology, and a synthetic or personalistic one. The personalistic strain is evinced especially in the fact that the notion of *disposition*—long banished from psychology—comes into its own once more. The variations of human character arise not through the differences of their respective particular phenomena or processes of consciousness, but from differences of the basic, potential, constant tendencies of persons. Whenever we talk about intelligence, character, temperament, etc., we mean these dispositional traits, of which the particular contents and courses of consciousness are merely expressions and symptoms. The teleological and psychophysically neutral character of these dispositions is correctly conceived; likewise they are duly distinguished from the old "faculties," in that they are not thought to be isolated, mutually independent entities, but mere relative aspects of the purposive structure of the person. But methodically this isolation is carried farther than was really consistent with the fundamental personalistic view. In particular, I still thought it possible, at that time, to render a picture of the personality of an individual *psychographically,* i.e., through his special traits—as they may be derived through observation, experiment, etc. I did indeed repudiate the idea that a mere mosaic juxtaposition of these elements could in itself produce the picture; not a summation, but a synthetic process was to yield the result. But still, the creation of a synthesis always supposes the priority of elements, from which, by some principle of correlation and the establishment of hierarchical, increasingly close relationships, the whole is to be derived. I did not see clearly then what I realize now (cf. p. 380 of this essay): that a genuinely personalistic psychology must transcend not only analytic but even synthetic method.

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[23] See footnote 10.

The necessity and fertility of the new concept of disposition was brought home to me particularly in one special problem, which began in those years to absorb my interest and has done so to the present day. That is the problem of *intelligence,* which—coming to the fore through practical necessity—required the most thorough attention from both theoretical and applied psychology. A summarizing survey, which I offered to the Psychological Congress of Berlin in the year 1911, entitled *Psychologische Methoden der Intelligenzprüfung,*[24] grew into a book, which later, for its third edition in 1920, was completely rewritten and considerably amplified, in fact was expanded to the proportions of a monograph, *Die Intelligence der Kinder und Jugendlichen und die Methoden ihrer Untersuchung.* [25] This book is the center of a widely diversified research carried on by myself and my students at Breslau and Hamburg, whose separate phases may be traced through various special articles in periodicals and later in the *Hamburger Arbeiten zur Begabungsforschung.*

The last years in Breslau had brought the study of *school-children* more and more into the foreground of my activity. The problem of intelligence, just referred to, was only one among several which I explored at that time, and of which I might further mention that of childish creativity (in composition, free-hand drawing, modelling, etc.). The relationship between this work and strictly *pedagogical* ambitions grew steadily closer, the latter being derived not only from psychology, but also from a lively immediate interest in questions of educational reform. Thus I took active part in the programs of the School-Reform League and its congresses, as well as the Pedagogical Groups of the undergraduates, designed to arouse in our young collegiates some consciousness of their future educational responsibilities. In the year 1913 both of the above-named societies met in Breslau, and the organization of their meetings was left partly to me. On the subject of one of their congresses, Comparative Studies of Youth of Both Sexes ("Vergleichende Jugendkunde beider Geschlechter"), I contributed an address, and arranged a psychological exhibit.

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[24] Die psychologischen Methoden der Intelligenzprüfung und deren Anwendung an Schulkindern. (*Ber. ü. d. V. Kong. f. exper. Psychol., Berlin.*) Leipzig: Barth, 1912. Also separate reprint, Leipzig: Barth, 1912. Pp. 106. (See also footnote 41.)

[25] See footnote 10.

In 1914 the *Psychologie der frühen Kindheit*[26] was published. Originally conceived merely as a contribution to the collection, *Wissenschaft und Bildung,* the manuscript grew under my hands to such proportions that it had to appear separately. Since there was no immediate prospect of continuing the monographs in collaboration with my wife, this inclusive presentation was at least to make use of some part of our vast diary-materials; but all the rest of the literature on the subject was consulted as well.

The book differs from its predecessors, especially from Preyer, mainly in that it goes as far as the sixth year, and in that it traces not so much the physiological as the strictly psychological developments of the infant. Although necessarily the distinctive functions—perception and memory, play and imagination, speech, emotional and volitional life—are treated here in separate chapters, yet a very definite attempt was made to relate all these particulars organically to the evolving personality. If one may judge by the number of editions and translations, this book has gained the greatest number of friends of all my publications.

The outbreak of the War disrupted a community of work at the Breslau Seminary, which was just at its height. And it was not merely a professional community: bonds of friendship held me to a long list of my students. Almost all of them went to War; only a few returned. Among those who fell were several on whom I had pinned particular hopes. Perhaps this circumstance contributed to the possibility of my decision, in 1915, to leave Breslau, which had become so dear to me.

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[26] See footnote 16.

A larger sphere of influence opened for me at Hamburg. Here Ernst Meumann had died in 1915. His place on the *Zeitschrift für pädagogische Psychologie,* which he had conducted together with Scheiber, I occupied at once; in 1916 I became his successor to the Hamburg professorship.

There was then no University at Hamburg, only the "Colonial Institute" and the "General Lectures," for which I, as sole professor had to represent the entire fields of philosophy, psychology, and pedagogy. I missed the intercourse with my students to which I was accustomed; on the other hand, I found a different circle of auditors and collaborators among the Hamburg teachers of various types of schools, who were greatly interested in psychology. Above all, I had at my command here an institute with other possibilities for scientific research than the little Breslau seminary.

The question of founding a university had long been a matter of dispute at Hamburg, and remained so even to 1919. Shortly after the collapse, the university, for which especially Mayor von Melle had fought against a large and influential faction, was finally established.

In a self-portrayal, it may be seemly enough to mention the part which I myself played in the ultimate foundation of the university. When, in November 1918, the military multitudes streamed homeward, I suddenly, in a sleepless night, conceived the idea: here are all the student sons of Hamburg families returning; these can be held in their native city through an emergency measure. The next day I suggested to the other professors of the Colonial Institute and the General Lectures that we professors should offer university courses, *privately,* to the returned boys; the suggestion met with approval, and in 1919 the courses, though without any official sanction, were already under way. The attendance was astonishing; the need had been demonstrated, and within a short time we succeeded in replacing the private enterprise by a state university.

The development of the Hamburg Psychological Institute, since the beginning of my activities there, and all the details of its productions, may the more easily be dispensed with here, as there are two special accounts of the history of the Institute down to 1925.[27] [28]

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[27] Das psychologische Laboratorium der Hamburgischen Universität. Gesamtbericht über seine Entwicklung und seine gegenwärtigen Arbeitsgebiete. *Zsch. f. päd. Psychol.,* 1922, **23**, 161-. Also Leipzig: Quelle u. Meyer, 1922. Pp. 40.

[28] Aus dreijähriger Arbeit des Hamburger psychologischen Laboratoriums.

Pure and applied psychology were pursued there; the *Hamburger Arbeiten zur Begabungsforschung* were issued there; a department of psychotechnics was presently added; problems of instruction, education, and vocational guidance were discussed in collaboration with the School Board, the teaching staff, the Labor Bureau and the Juvenile Bureau.

My own interests as well as the situation which I found in Hamburg caused the study of youth to become one of our main themes. A little prospectus-like work, *Jugendkunde als Kulturforderung,*[29] which I published at the beginning of my Hamburg career, though it was really designed merely to indicate the importance of this new type of research, laid down certain general outlines of our work at the Institute.

Thus the two problems principally stressed in the essay— selection of students and determination of vocational talents—have become subjects of perennial interest for us. In particular, we could, after many years' experience, base the selection of superior students who passed from common school to the higher institutions, on psychological experiments and methods of observation; and, at the present day, the "Hamburg procedure" is known as probably the most highly developed in all Germany.[30] [31] [32]

In regard to our theoretical work with the ability-problem and especially the intelligence-problem, [33] I would make but one remark about the general course which was pursued during the decade. Its starting-point was the conception of intelligence as a purposively oriented, personal disposition, that of "general intellectual adaptability to new tasks and conditions of life." From this teleological

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Bericht über die pädagogisch-psychologische Tätigkeit des Instituts 1922-25. *Zsch. f. päd. Psychol.,* 1925, **26**, 289-307.

[29] Die Jugendkunde als Kulturforderung. Mit besonderer Berücksichtigung des Begabungsproblems. *Zsch. f. päd. Psychol.,* 1916, **17**, 273-. Also Leipzig: Quelle u. Meyer, 1916.

[30] R. Peter & W. Stern [Eds.] Die Auslese befähigter Volksschüler in Hamburg. (No. 1 der Hamburger Arbeiten zur Begabungsforschung. *Zsch. f. angew. Psychol.,* Beiheft 18.) Leipzig: Barth, 1919. 2nd ed., 1922. Pp. 161.

[31] Die Psychologie und die Schülerauslese. Leipzig: Barth, 1920. Pp. 69.

[32] Probleme der Schülerauslese. Vortrag. Leipzig: Quelle u. Meyer, 1926. Pp. 50.

[33] Cf. especially Grundgedanken der personalistischen Philosophie. (Philosophische Vorträge der Kantgesellschaft, No. 20.) Berlin: Reuter u. Reichard, 1918. Pp. 54.

point of view, special importance was attached to the "accomplishment," which was the aim of certain intellectual tasks. Therefore our next ambition had to be a maximally exact determination of such intellectual feats; and a whole arsenal full of tests for this matter have since then been invented, established, and applied. (Together with Otto Wiegmann I have issued a handbook which collects and classifies all these methods). [34]

But all these discoveries did not take us yet into the real heart of psychology, so long as it was unknown through what *psychical* process the solutions were attained, and in what way the special contributive factors: attention, memory, thought, etc., were involved. This emphasised the necessity of a thorough psychological treatment of the *mental processes* that respond to the intelligence tests—a general problem with which our professional circle has latterly been constantly occupied.

On the other hand, I have lately come to the conclusion that, in the course of such specialized treatment, the factor of intelligence is presented too much in isolation, as an apparently independent disposition, without due regard to its original membership in the personality as a whole. Therefore we must pay more attention to its "personalistic anchorage," and therewith to its fundamental relatedness to the impulsive, volitional, and practical life.

Another central theme of our juvenile studies at Hamburg is the psychology of adolescence. The survey volume of the subject which I had planned is not done yet; there was still too great a dearth of special preliminary studies, and I did not want to incur the failing which other scholars have not always quite avoided: to try to outline the enormous field of these researches by means of a few sweeping basic ideas. Certainly it is necessary—particularly so, perhaps, at the present stage—to orient one's psychological notions by certain philosophical premises; and I myself have attempted, in short articles, to derive such premises from the psychology of personalism, in treating adolescence as "the time of the discovery of values, and of adjustment between the self-value and the world-values."[35] But, if one would avoid getting stuck in the schema, one must be familiar at the same time with the exceeding variegation of the several phenomena, and approach their discovery and interpretation by the most diverse methods, to sound the secrets of this epoch of life. For this reason I have lately treated separate problems in monographic form: the beginnings of adolescence, *Anfange der Reifezeit,*[36] on the basis of a diary-analysis, and the sad theme of juvenile witnesses in morality-lawsuits, *Jugendliche Zuegen in Sittlichkeitsprozessen,*[37] for which I, as expert advisor, had access to court material.

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[34] With Otto Wiegmann. Methodensammlung zur Intelligenzprüfung von Kindern und Jugendlichen. Leipzig: Barth, 1920. 3rd ed., 1926. Pp. 541.

[35] Drei Vorträge zur Psychologie der reifenden Jugend. In Erziehungsprobleme der Reifezeit, a series edited by H. Küster. Leipzig: Quelle u. Meyer, 1925.

During the early part of the Hamburg period I was able once more—after an interval of ten years—to bring some *philosophical* works to their conclusion; the second volume of the *magnum opus,* and two shorter works appeared in 1917-18.

The brief sketch of the total system which I offered at that time in a lecture to the *Kantgesellschaft*[38] characterizes the intermediate stage belonging to that epoch: I had already transcended the one-sided biologistic concept of the person, through my introduction of the notion of "Introception," and my emphasis on the problem of consciousness; but the way toward a philosophy of value and meaning—and thus toward the conquest of spiritual and cultural scientific fields—was, for the most part, still ahead of me.

The other two treatises belong together in that they both concern *human* personality; but they employ opposite methods.

The book which bears the title, *Meschliche Persönlichkeit,*[39] proceeds from the fundamental characteristics of human personality to the details of their being and significance. The new crucial ideas of this essay are "convergence," "introception," and the interpretation of psychical *experience* through the psychologically neutral life of the person.

A smaller article was originally conceived as a part of this book, but was then severed from it and separately published under the title, *"Die Psychologie und der Personalismus."*[40] Here at last I attempt consciously to bridge the chasm between my two fields of labor, by demonstrating that psychological research itself leads one to a personalistic general outlook; here the doctrine of critical personalism "is to be worked out through the results, ideals, and difficulties of present-day psychology."

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[36] Anfänge der Reifezeit. Ein Knabentagebuch in psychologischen Bearbeitung. (Reifende Jugend I.) Leipzig: Quelle u. Meyer, 1926. Pp. 125.

[37] Jugendliche Zeugen in Sittlichkeitsprozessen, ihre Behandlung und psychologische Begutachtung. (Reifende Jugend II.) Leipzig: Quelle u. Meyer, 1926. Pp. 193.

[38] Grundgedanken der personalistischen Philosophie. (Philosophische Vorträge der der Kantgesellschaft No. 20.) Berlin: Reuther u. Reichard, 1918. Pp. 54.

[39] Die menschliche Persönlichkeit. Volume II of Person und Sache. System des kritischen Personalismus. Leipzig: Barth, 1918. Pp. 272. (3rd ed.) 1923.

[40] Die Psychologie und der Personalismus. *Zsch. f. Psychol.,* 1917, **78.** Also Leipzig: Barth, 1917. Pp. 54.

The content of both books will be discussed in the next section.

Several years passed, during which I was more or less preoccupied, besides my current special researches, with the revision of previous works; thus in 1920 the *Intelligenz der Kinder und Jugendlichen* appeared in a totally rewritten edition,[41] and the editions of *Psychologie der frühen Kindheit,* of 1923 and 1926, were extensively revised.

Yet every available moment of leisure was devoted to the continuation of the philosophical system, and thus the third (conclusive) volume, *Wertphilosophie,* was ready for publication six years after the second. [42]

Today I regard this book as the truly characteristic and at the same time most complete expression of the personalistic attitude toward nature and life, and should regret to have it always figure merely as "volume three," not as a self-sufficient work. Perhaps, also, the designation "Philosophy of Value" is not entirely unequivocal; value, for me, is synonymous with "metaphysical significance;" thus the book presents another statement of personalistic metaphysics not, like the first volume, as theory of being, but, more profoundly, as *theory of meaning.* It is only here that the spiritual, historical, and normative sciences receive their philosophical foundation .

The total presentation of the system is exhausted in these three volumes, but not, I hope, my task of detailed elaboration. The next undertaking which I dimly see before me, is that of complementing the personalistic philosophy by a *science of personalistics.* A world-philosophy is a matter of faith and can be neither proved nor refuted. But now it behooves me to show—even to him who does not confess my metaphysical faith—that the personalistic conviction contains certain principles of research, explanation and understanding on which a *science* of human personality can be based. Such a personalism has something to do with determining the psychologies of the future.

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[41] Die Intelligenz der Kinder und Jugendlichen und die Methoden ihrer Untersuchung. (3rd ed. of reference given in footnote 24.) Leipzig: Barth, 1920. Pp. 335.

[42] Wertphilosophie. Volume III of Person und Sache. (With Author's note to Volumes I, II, and III.) System des kritischen Personalismus. Leipzig: Barth, 1924. Pp. 474.

IV. The Personalistic System[43]

This, finally, is to be an attempt to present certain salient features of the total system in their logical relationships, and at the same time to indicate the gradual evolution of the personalistic idea, as it grew through the twenty years of labor on the three volumes.

"Now the system has three main divisions, which correspond to the three volumes: general philosophy and cosmology, the doctrine of man, and the doctrine of value. With these it tries to lay the foundations for a new conception of the world and offer a norm for the conduct of life. At the same time this system wants absolutely to be regarded as an open one; however much it strives to establish the foundations dogmatically and critically, it must often be content in matters of detail to assert an intuitively apprehended conviction whose scientific proof and justification has to be left to the future, or by a new mental attitude to offer opportunities for further thinking, modification and revision.[44]

The basic premises of philosophical personalism remained intact during all the mutations of those twenty years: namely that cosmology, science, and life must have their roots in *metaphysics,* and that the fundamental category for the present epoch and for the next must be that of the "person."

By metaphysics, however, the personalist does not mean the possession of complete truth, but rather a "seeking faith in being and value" (III). "I believe in a world which is at once existent and valuable; and I *seek* this world." This proposition is the *a priori* of all philosophizing, research, and challenge. The fact that metaphysical faith is a groping faith distinguishes it from religion, which is inwardly at rest. Because metaphysics is seeking, it must also be critical; i.e., it must account for every step whereby it approaches nearer to the being and value of the world; it must inquire whether and how well its own categories, theses, and hypotheses correspond to the ultimate premises of its faith, which in turn, of course, are irrational arch-postulates and thus are prior to any possible critique. Finally, metaphysics, because it is seeking, must be *progressive;* every metaphysical system is but a resting-place along an endless road, and therefore cannot exclude from its own being its conditionality and temporal limitation.

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[43] The three volumes of the system will be referred to below, for the sake of brevity, as I, II, and III. (I) Person und Sache. System der philosophischen Weltanschauung. Band I: Ableitung und Grundlehre. Leipzig: Barth, 1906. Pp. 434. 2nd ed., with Author's note to Volumes I, II, and III under the title: Person und Sache: System des kritischen Personalismus. Band I: Ableitung und Grundlehre des kritischen Personalismus. Leipzig: Barth, 1923. (II) Reference given in footnote 39. (III) Reference given in footnote 42.

[44] "From the Author's note to Volumes I, II, and III."

Here we must add a few words on the attitude of personalism toward epistemology.

The oft-repeated demand to let *critical* thought take the place of *creative* thought in philosophy is distinctly repudiated. Never can criticism precede creativity, let alone forbid it (i.e., deny us the right of metaphysical attempts altogether) ; but it must follow in the footsteps of invention, must control, justify, or rectify it.

I personally, indeed—despite the hypertrophy of the time—was more concerned to illumine the *material* content of the personalistic theory; yet the directive ideas toward a future personalistic theory of knowledge are contained in many passages of I and III.

We will take up four main points of this epistemology.

1) It is a *critical dogmatism,* i.e., the *a priori* of all cognition does not consist of acts which are themselves of an epistemological character, but consists of groundless postulates of faith. Only insofar as a man *believes* in a real and significant world, can knowledge be anything more than sense-illusion and a play of concepts. Consequently "critical" epistemology is not the kind that rejects dogmatism, but that which reflects upon its own postulates of faith and determines the truth-value of its insight through these. In place of the dichotomy of dogmatism and criticism, we should have one of good (critical) dogmatism, and bad (uncritical) dogmatism.

2) Personalistic epistemology is *realistic;* for its *a priori* faith posits a significant Being, which has reality outside the believing consciousness. The knowledge which seeks to realize this belief is indeed determined as to its *kind* by the general and differential character of the subject; in regard to its *intension,* however, it is always directed toward an object which transcends the subject and consciousness, whose being and nature are to be described in progressive, howbeit never-ending approximation.

3) Concerning *media of knowledge.* Sense-experience and rational thought—regarded for centuries as solely important—prove to be insufficient; not only singly, as they are employed in the one case by the empiricists, in the other by the rationalists, but also in conjunction, as Kant treated them. For both are essentially instruments of an impersonalistic understanding; the concretely individual

totality remains outside the sphere of their application. In this way a large class of possible objects of knowledge (especially historical and cultural, but also some objects of other sciences are rendered epistemologically homeless.

At this point, personalism meets with certain new tendencies, which attempt to do justice to the concretely holistic and personalistic aspects through different means of knowledge ("intuition," "insight"). But we are not content with a sheer opposition of these means to the afore-mentioned sorts. Our purpose is rather to establish a *hierarchy of cognitive principles* (III, Chap. 9); the "de-ipsized" cognition progresses by an anabasis through the three stages of lower intuition, conceptual abstraction, and higher intuition; beyond these, however, there is a way of knowing, whereby the self does not seek to eliminate itself, but to identify itself with its object. This is *sympathetic introception,* wherein the maximal approximation to the intrinsic being of personalities is attained.

4) The *categorial theory* of personalistic epistemology is opposed to that evaporation of the category of *substance,* which in the last analysis is aiming directly at substituting for this the category of *function.* The very separation of these two categories was the abstract product of impersonalistic epistemological tendencies; indeed, as long as any attempt was made to regard stark being as the substratum of coincident properties, on the one hand, and, on the other, to think of a functional connection among events as pure isolated relation, the coexistence of these two categories could not but become meaningless. If, however, this isolating abstractness is given up, then all substantiality proves to be nothing but the fountainhead and final goal of activities (and thus of functional relationships) and conversely all interrelations of actual occurrences necessarily point back toward actuating absolute (i.e., toward substances), from which they must originally derive their significance. Thus the two categories belong indivorcibly together; there is nothing but *substantial causality* and *effective* (functioning) *substance.* But these, in turn, are comprehensible only when they are individualized, and thus the category of *individuality* is the third, which blends with the other two in the constitution of the unitary and actual object of knowledge. By this interdependence of all these three categories, each with each, any one of them attains a character quite out of keeping with the orthodox notion; especially causality now comes to mean, primarily, purposive activity, and only secondarily, func-

tional relationships of events. Thus final cause becomes the fundamental form, efficient cause a derivative mode of the category of causality.

The fundamental lines of the *content* of my system may be briefly traced as follows:

The central idea is the category of the "Person." This, indeed, is the basic idea of all belief in souls and gods, which always did and always will control the thought of mankind. But critical personalism is distinguished from this "naïve personalism" by a completely different *conceptual construction* of the "person" and a methodical application of the category.

All real Being—such is our teaching—must be conceived in the form of persons. The defining property of "person" is *concrete, purposive activity;* the world consists of essences which are because they have effects; which are wholes, through the fact that they exemplify in themselves a significant manifold of parts; which are bearers of a teleological causality, in that the meaning of the totality determines the realization of its subordinate part-purposes; which are concrete and individual, in that they alone give significance and sense to all abstraction and generalization.

This conception of the category of "person" leaves every mere anthropism far behind; it is applicable to the human, the sub-human, the super-human, to the organic and the inorganic, to individual and societal forms—in so far as they are regarded in the light of the conceptions we have just indicated.

Furthermore, all three volumes keep in view the critical formulation of the person as an *unitas multiplex.* They reject the simple "soul-thing" as well as the purely aggregational structure of impersonalism. The world is to be comprehended neither through a process of refining out its ultimate elements, nor by joining elements together into complexes, but only through a *coordination* of everything that appears to us as elements and aggregates, with real, original, closed totalities, into which they resolve themselves and through which they may be understood. We arrive at real Being neither by analysis nor by synthesis, but only by "hypostasis." But, whereas formerly the never-resting tendencies to hypostatization proceeded without curb or critique, and conjured up a mythological chaos of personalistic fictions, our critical personalism demands that the "hypostatic method" be developed into a scientific procedure, to teach us *in what aspects of reality and by what criteria* we should look for the right and the duty of "personification." (I and III).

As a universal complement to the concept of "person," we have developed the concept of "thing." It applies to everything that is not whole, but merely aggregated, not purposive original activity, but a sphere of influence of foreign determinants; not concretely individudal, but abstractly valid; not absolute, but relative. The theory of things, or "impersonalism," is traced through the centuries as a great, pervasive, intellectual process, in cosmology, science, and ethical expression. The impersonalistic categories have their undeniable sphere of application, within which no union with naïve personalism is possible. It is only the new, critical formulation of personalism that enables us to transcend this dualism. That realm of the impersonalistic categories is itself a derivative concept, determined as to its significance only by deduction from the categories of personalism. By reason of this deduction ("teleomechanics") the thing-concept ceases to be a mere complement to that of the "person," and becomes a positive—though always secondary—integral part of the system.

Another metaphysical separation, namely that of *body and mind,* is also resolved by critical personalism. "Person" is as far from being identical with material substance as from identity with soul or consciousness or spirit. Every person as such is "psychophysically neutral," because "physical" and "psychical" are merely modes of expression, by which it is represented to others and to itself; its immediate functions and properties are "personal," and may embody within themselves as a perfect unity most various forms of physical and psychical material.

The doctrine of the *psychophysical neutrality of Being* is carried out in Volume I as a general principle of cosmology, by which the cosmic dualism of spirit and matter is transcended by a personalistic monism. Volume II draws the inferences for Man. Volume III gives the fundamental principle an axiological turn: even the characteristic values of material things on the one hand, mental and spiritual things, on the other, are derivative in relation to the intrinsic values of the self.

In referring, just above, to the system as a "personalistic monism," I meant to imply the unity of the *personalistic principle,* not the singleness of personality-substance. Rather we find here—consistently throughout the three volumes—the idea of a *hierarchy* of persons: for each person—being not simple, but *unitas multiplex*—can contain other persons within itself, and be in its turn an integral part of higher personal unities, till this hierarchical system finds its upper limit in the universal divine all-person ("personalistic pantheism"). The postulate, that a being can be at once a whole and part of other wholes, indeed must be such, lends to the relations of persons with one another a characterizing color, and, in application to man, becomes a decisive idea for theory of value and of norms.

The basic lines of critical personalism, as sketched above, form the background against which we may mark a noticeable *development of thought* and *transference of emphasis* from volume to volume. Only in retrospect is it possible to trace a unified tendency through all these mutations, which we are about to recount; the direction of this tendency is from a primarily *naturalistic* to a primarily *interpretational* attitude in philosophy; my earliest interest being centered in explanations of the given, but yielding more and more, in the course of time, to an interest in the appreciation of its sense.

The main thing I sought to present in the first volume was a metaphysics of natural being. Not that this volume is limited to the things which commonly are relegated to the natural sciences; for it seeks also to include the sciences of mind, history, and culture. Yet its problems are chiefly such as are formulated for us by natural philosophy and natural science, which therefore could be paralleled in the special sciences; and their alternative solutions, too, had been set forth in science more clearly and logically as definite opposites, than anywhere else, and could therefore serve as a point of departure for new personalistic solutions. In particular, it was the sphere of life-processes (that is to say, the sphere of biological problems in the widest sense) that served me as such an initial point. For this realm was in close relationship with the inorganic natural objects of physico-chemistry on the one hand; with human life, and thus with psychological and ethical matters, on the other; and, finally, with the super-individual life-processes of culture and history; and it was possible to begin simply with organic processes and gradually take in all other domains of the cosmos.

This was immediately apparent in the postulation of the fundamental alternative. For, although a number of intellectual motives may have influenced the choice of the dichotomy of "person" and "thing," these two correlative concepts originally took their color from that alternative which has dominated philosophy since the classical age: *mechanism / teleology.* The question at issue has always been, whether the living individual is to be taken as a mechanical complex of all its parts, or as the bearer of a purposive immanent activity. But the question had long since ceased to be confined within the bounds of the biological realm, and had become a problem of *natural causality as such.*

Thus personalism was faced with a threefold task. In the first place, it had to prove that the alternative: teleology / mechanism is not identical with the apposition: psychical / physical, but lies perpendicular to it, so to speak, and therefore has a perfectly independent meaning of its own. Furthermore, we demonstrated that this meaning is indeed a *universal* one; that the dichotomy is carried out under the most various names and in the most diverse fields of knowledge, and that therefore our aim must be a universal solution, not one restricted to the narrow field of biology.[45] And, finally, our doctrine essays to find the solution itself.

This solution is attained by two steps.

The first step is phenomenological and explanatory treatment of the whole teleology problem in itself. Here the various conceptions of "teleology" had to be separated out, whereby I discovered that the profound and general aversion which is often maintained against the notion and the name relates only to the naïve, external "teleology of intentions," but not to immanent or "disposition"-teleology (instinct of self-preservation, evolutionary tendency); in fact, it could be proved that even apparently anti-teleological attitudes (as, for instance, the Darwinian) could not be maintained at all without recourse to some disguised immanence-teleology. Thus indirectly the indispensability of a teleological explanation of the world was set forth.

Particularly in the *theory of evolution* I have here set up a hypothesis which seems to me to fit the facts better than any other doctrine. On the basis of the hierarchistic notion, the factor of *birth* is carried over from the single individual to the *species* (which is conceived as a personalistic super-unity). The biological species possesses—besides its racial instinct of self-preservation, which is expressed in heredity—a tendency to self-development, which drives it at certain times to the genesis of new species. Thus in the thesis of the *“birth of species”* we retain the principle of descent; but, in place of the mushy progress by minimal variations (Darwin), we have the alternation of constancy and catastrophic bearing of new forms. This hypothesis accounts for “mutations”; also it explains the absence of “missing links,” since these necessarily have an unstable—we might say, racially embryonic—character of transitional stages.

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[45] This distinguishes personalism from vitalism, its partner in the fight against a mechanistic explanation of life-processes. But vitalism limits its assumption of a purposive causality to organic individuals; and it conceives this causality through a special factor (vital impulse, dominant, soul-factor)—which immortalizes the dualism of teleological and mechanistic causation. In this, vitalism is "naïvely personalistic."

Unfortunately, the hypothesis of the birth of species has not, to this day, come to the notice of biologists.

The second step takes us into *“teleomechanics.”*

Between naïve personalism with its belief in separate souls, gods, and vital forces, and impersonalism which would make the whole world a senseless system of elementary uniformities and treat all life as a physico-chemical aggregate, there can never be any conciliation, as I have already pointed out. But critical personalism can transcend the opposition. For the person is no longer a separate thing *besides* other entities, which have mechanically-objective structures; but an actuality is a “person” *insofar* as it is a whole, which maintains itself in conformity with its immanent purposes, develops, articulates itself; and is a “thing” *insofar* as it is a part of other persons, and obeys the activating principles of these superior entities. *Mechanical uniformities are merely by-products of personal activities,* are derived from the latter and no longer offer a genuine complement to it. This relationship between the personalistic and the objective realms is called in Volume I “teleomechanical parallelism”;
microcosmic: the concepts of “convergence” and “introception.”
but the name is not very well chosen, because it seems to imply a value-equality of the two terms in the relation of parallelism, whereas I meant to postulate from the beginning the primary character of personalistic being and the secondary nature of the mechanistic aspect. Indeed, for just this reason I attempted at the time to sketch the outlines of a “teleomechanics,” i.e., to *deduce* the principles of the objective attitude: magnitude, number, uniformity, etc., from those of teleology—that is, to go the opposite way from that of mechanism, which seeks to explain life and significant existence in terms of the objective categories.

The teleomechanistic notion causes critical personalism to affect *mathematics* and the *exact sciences*. We will indicate two points in this connection:

1) The category of *equality* is the fundamental assumption of these sciences; for mathematics deals only with the formal conditions for equivalence, physics and chemistry with material similarities ("laws"). But what is a premise to the special sciences becomes a theorem for philosophy. Equality is not complete coincidence, but *indiscernibility of differences of meaning;* thus it is an index of something that produces meanings, and lies deeper than the terms of the equation themselves. Only because there are centers of meaning, which in their own natures are incommensurate, other things become significant in relation to these, and become interchangeable with respect to their significance (commensurate). The formal side of this fact forms the real foundation of mathematics (thus in future one might develop a "teleomathematics"). In respect to material nature, the concept of "natural law" may be both justified and limited. Uniformity is to be found there and only there where a self-determined actuality produces the rule of its own functioning. The number, extent, even the genesis and decline of natural laws are determined by the number, extent, and development of personalistic entities of different magnitudes.

2) The *substantial structure* of the inorganic world. Here the latest tendencies of exact research unexpectedly come to meet the personalist's world-view. For in place of the mere togetherness of ultimate elements they assume the *hierarchical superposition of individualized active substances.* From the atom, the known hierarchy at present reaches upward to the molecule and the crystal, downward to the electron; lower micro-stages may, perchance, have to be assumed in the future. The atom, as a "solar system in miniature," even becomes the bearer of an immanent, discontinuous causality, which has a curiously personalistic flavor. One may balk at the transferred term, "persons," for such stages of substances—what is important is the fact that the inorganic microcosm fits without friction into the hierarchical total system of critical personalism.

The first volume was an attempt to trace the thread of personalistic thinking as far as the establishment of the *person as such,* regardless of its magnitude or level of actuality. In the second volume, which applies the concept of the "person" to man, we no longer deal exclusively in generalities, but on the contrary must seek to master the wealth of data and the immediate certainty of experiences which a human being has of his own personality; but it is just in the comprehension of this empirical material that the previously acquired metaphysical categories render us indispensable services. They may be completely taken over, but they prove to be no longer solely sufficient. The primarily naturalistic point of view requires some supplement; and in this supplement lies the critical intellectual advance of the second volume. It is the step from the *biological to the microcosmic datum,* from the "person" to the "personality."

We discover this new truth through our own nature: that which heretofore has been regarded as the person, namely the reality of a purposive active whole, is not in itself our full being; the latter must rather be taken to have some sense which refers it to the totality of all being, without, however, any loss of our personal self-identity. Two new concepts are introduced at this point which express these two aspects of man's relation to the world, the biological and the the microcosmic: the concepts of "convergence" and "introception."

The question regarding the *conditions* for human Being and life are answered by means of the *convergence-postulate,* which is supposed to overcome the old conflict of "nativistic" and "empirical" explanation. Man is—physically and psychically—neither a creature of stark and absolute innate properties, nor a passive product of environmental influences; the inner determinants of his life have rather the character of *"dispositions,"* i.e., purposively inclined, but not yet unequivocally confirmed tendencies and preparations for action, whose field of operation is definitely marked out only with the cooperation of external factors, as an actual personal career. But the dispositions are not independent separate "faculties"; they are mere partial rays of the personalistic unity, that is, of a unified total tendency or entelechy. The environmental factors, in their turn, are never simply coercive forces, under whose pressure the person is cast into a certain mold, but they are stimuli, raw materials, points of attack, and collaborators of just that inner entelechy. The interaction, the "convergence" of these two groups of conditioning factors is the occurrence of the real person.

But this outer world is for a human being not only a *condition,* but also *a part of the meaning* of his life—and therewith we transcend the merely biological point of view. Although the world, as a causal factor, may influence the personal entelechy from without, and in convergence with it may determine the actual course of events, it is none the less *internally* related to this entelechy in a perfectly novel sense. For it (the outer world), too, is a system of significant wholes, and therefore of ends in themselves; therefore it is related to the human being who stands within it, and, furthermore, it is the very thing which permits the punctual selfhood of the human personality to expand itself into a *microcosmic self.* The world, insofar as it embodies significant essentiality, does not only set off the individual, i.e., play the part of his material and complement, but it enters into the unity of his own system of ends the objective world-meanings, ends and values are utilized by man as factors in his own selfhood and his personalistic self-determination. This meaningful "intaking" of the not-self by the self is *introception.* Only at this point can we comprehend the fact that man is not bound to meet the objective purposes of the world with the alternative: either to combat it as the opponent of his selfhood, possibly to utilize it—or else, to yield to its ruling forces and sacrifice himself, i.e., to depersonalize himself; rather there is a *common assent,* actually a conjoining of both.

But since introception cannot be exhausted in particular vital acts, but means the constant, never-ending task of realizing values, man in relation to it has not merely actual existence, but an ideal *vocation;* he is not only biological person but microcosmic personality. Thus in Volume II we meet with the basic theme of personalistic ethics, which is developed in Volume III.

Here it behooves us to interpolate that complex of ideas which relates to the personalistic foundation of psychology. This also is touched upon in Volume II, and in the little companion-work,[46] deepened in Volume III, and is to be systematically developed in the near future by a reduction of all psychology to "personalistics."

By "personalistics" I would denote—in distinction from the philosophy of "personalism"—the *science* of the human person. Such a science is just on the point of evolving, and promises to be a common propaedeutic to all those disciplines which are concerned with humanity. Humanity was hopelessly torn asunder scientifically, in that it was treated by some disciplines with respect to its bodily aspects, by others for its consciousness, and by yet a third group in regard to its cultural expression. A retrospective agglutination of the previously separated parts (such as physiological psychology, a physiology of language, etc.) did not suffice to repair the damage.

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[46] See footnote 40.

The only thing that can help us here is a science that furnishes the common—yes, really the generating—categories of all sciences of humanity. The physiological, psychological, and historical categories of reflection and research are not self-sufficient, they isolate artificially things which have meaning and sense only in their relation to the physically-psychically-culturally *neutral* totality of the human person; therefore all those categories need to be determined by specification of the profounder categories of personalistics.

This idea, applied to psychology, may be tackled from two vantage points; both, as already suggested, have been occupied by me.

The first way proceeds "from below upward,"[47] that is to say, it starts from the multiplicity of psychical phenomena, which are of course the prime object of special psychological research. But from this multiplicity itself the unity of the person can never be deduced; rather we must introduce as guiding thought the category of the "multi-unity" itself, and, more precisely, in the form of successively superimposed *projection-fields* (phenomena, acts, dispositions, ego).

The lowest level of this multiplicity is represented by "phenomena" (data of consciousness), whose relationships can be explained, not through abstract mechanical laws of association, but only through unitary personalistic "acts." The acts in their turn are indeed momentary units of activity of the person, but in order to enter into temporal relations with each other they require the unifying foundation of its efficient faculty, or "disposition." But dispositions as yet must be postulated in the plural, as they represent the potentiality for the separate partial purposes of the person; thus they again demand reduction, this time to the unitary disposition of the ego, or the "entelechy." The term "psychic" applies unequivocally only to the lowest level of phenomena; acts and disposition may indeed be directed toward the psychical, but are in themselves of a metaphysical, immediately personalistic character. Therefore it is possible to identify them with those acts and dispositions which relate to the other phenomenal aspect of the person, namely the physical data. Hence, the further we go from the surface realm of the data of consciousness and delve into genuinely personalistic depths, the more we desert the sphere of distinctions of physical and psychical things, and approach that of *psychophysical neutrality*.

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[47] See footnote 40.

The other way proceeds from above downward, and derives the meaning and structure of the psychical from the category of "the person."

1) *The problem of meaning.* In an autonomous psychology there could be no question of the "meaning" of consciousness, of experience, there would only be description. Only as psychology loses its autonomy, and its object, "consciousness," loses its independent existence, both acquire "meaning." The person gives a certain part of itself (the "pugnacious" part) and "internal rendering," in order to use this self-awareness as a weapon and as a mirror. Thus "meaning" is two-fold: there is service-meaning (weapon) and ray-meaning (mirror). But since consciousness always yields only *one* item of the indivisible personal life, it is not a smooth, direct mirror of external things and of the self proper, but only an eternal approximation to both. "The significance of experience lies in its transcendence." The way from the self-experience to the true "ego-in-itself" is just as far (namely, infinitely) as that from the object-experience to the "object-in-itself." Consequently the events of consciousness are always images and *virtual* images (*Spiegelungen und Vorspiegelungen*) at once. The unconscious, too, may now be assigned its proper place; it is not some sort of secret power *beside* consciousness, but is the aboriginal life—prior to all awareness—the personalistic life itself.

From the problem of significance we must come by natural consequence to the problem of *interpretation;* for now there is some sense in asking what essential personalistic attributes certain psychic conditions "signify." Our present stock of interpretations of expression, handwriting, dream, tests, and other things, are only timid beginnings of a scientific theory of semantics, which promises to become one of the most important departments of personalistic psychology.

2) *The problem of structure.* The person, as *unitas multiplex,* is structured, and consequently so are its psychic "internal renderings." From the very beginning everything is given only through structures: psychical "elements" simply do not exist. Not only "destructive analysis," but also "creative synthesis," which would produce the novel totality out of the elements, is inadequate. In place of analysis and synthesis we employ another pair of concepts, which preserves the trait of wholeness: everything that *"stands forth"* from the whole, nevertheless remains *"embedded"* in it, despite its isolation, and receives from this relationship its sense and

order. Between maximal isolation and maximal embeddedness we find all mediate steps; the "Gestalten" which a certain departure in contemporary psychology treats as absolute represents only a limiting case of certain isolations of structural characters.

Another problem of structure relates to the matter of *degrees of essentiality*. The psychical is incomprehensible, as long as we try—in conformity to the naturalistic levelling tendency—to regard everything as equally important, equally significant (or insignificant). In the totality of the person there are higher and lower functions, shallower and deeper levels, truer and falser aspects. Of this, too, the latest researches show some glimmerings; but the various degrees of essentiality will become scientifically explicable only through a philosophical fundamental conception of the person.

My leanings toward the philosophy of meaning, which are apparent in the second volume, are followed in detail throughout the third volume. At the same time, the new outlook, which in the second volume was applied only to the human individual, is now expanded into a *microcosmic* view—only to comprehend Man, as the theory comes back to him in the end, in his *microcosmic* significance.

In the concept of *value,* the significance of Being receives its most general expression. The personalistic creed, "The world is a hierarchy of persons," is now taken not merely as a proposition about the mere factual and conditioned features of the world, but a confession of ultimate meanings and meaning-relations. The world is regarded as a system of *intrinsic values,* each of which exists only in and for itself, carries its fundamental importance in itself, and therefore can never be derived from some other value-condition, borrowed from elsewhere, but each of which, none the less, stands at the same time in some relation to the totality of all other intrinsic values, and thus receives its special world-conditioned place in the hierarchy. This "at the same time" is the fundamental mystery of the existence of all value; it is the *dialectic of the concrete,* which personalism offers in place of the *dialectic of the abstract* of Hegel.

The contrast of "concrete" and "abstract" was indeed given from the outset in the systematic alternative of person versus thing, but its decisive importance appears only in the philosophy of value. The "thing" now appears in the form of the *abstract idea.* The question concerns the ultimate autonomous bearers of value: are these bearers concrete totalities or abstract generalities? *"Persons"* or *"Ideas?"*

The personalistic theory of value seeks to show that the concept of intrinsic value must coincide with that of genuine Being, which latter, however, cannot be thought other than concrete. That has intrinsic value which, as a totality, practices self-determination, and through this self-determination tends constantly to realize its own real Being. This is true only of "persons." Everything abstract, general—and therewithal *every idea of value*—flows from the unique original significance of a concrete whole, which radiates forth upon everything appertaining to that whole, as general—and now, normatively valid—significance. That these concrete bearers of intrinsic value, from which all ideas of value only derive their glamor and virtue, are not identical with human individuals, follows logically from the principles of the hierarchy; personalistic Being at any stage whatever is capable of radiating its intrinsic value as "ideas." For us human individuals those ideas become normative, which have their origin in higher, superindividual wholes (Nation, Humanity, God).

It is evident that by this turn personalism acquires a new battlefront. For that impersonalism against which it is directed is no longer merely *mechanism* in the explanation of nature, spirit, and "Kultur," but is at the same time the philosophy of *abstract idealism;*[48] personalism, as opposed to these, represents for its part a *concrete idealism.* Our reverence for the great contribution of Platonism (in the widest sense, including also Kant) to the thought of humankind, should not prevent us from admitting that in this decisive matter we find it wanting.

There has indeed always been a concrete idealism of value, which, undisturbed by the abstractions of philosophy, determined the convictions and norms of values for the life of humanity. But it existed outside philosophy, or merely reached into its domain with occasional and uncertain feelers (as for instance in Kant). Its sphere was that of religion and myth, art and practical life; but since it lacked critical, philosophical control, its close attachment to the concrete led to arbitrary personifications and deifications, to canonization of haphazard forms of value, to absolutistic treatment of values which are really of a derivative character. In short, it was naïve personalism, with all the short-comings of which science can easily enough convict it.

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[48] Previous page: Incidentally, these two apparently disparate forms of impersonalism, which personalism combats, have a close inner relationship. Is it not mere chance that Plato as well as Kant believed in the ultimate nature of *valid ideas* in the sphere of value, and in philosophy of nature inclined toward a mathematical-mechanical point of view; both employ abstractly rational and impersonal categories of thought.

A philosophical theory of value must transcend this point of view, but not by putting impersonalism in its place—rather by substituting critical reflection for naïve acceptance within personalism itself. The hypostatic method (see above) will point the way to those personalistic essences, which we must recognize as bearers of ultimate intrinsic values, and which in their turn become the points of origin for all derivative values, including the abstractly idealistic.

The hierarchy of intrinsic values, which is won in this fashion, is just beginning to appear in outline. Already the human individual personality, and the super-individual personalistic unities of the family, the nation, humanity, are definitely emerging; what other forms within and without the human realm, what astronomical, biological, or inorganic configurations may be regarded as genuine unities capable of self-determination, and therefore as intrinsic values, can be decided only by slow scientific labor based on the hypostatic method. The conviction, on the other hand, that the upper limit of the hierarchy of values is not a supreme idea of Good, by the concrete divine All-Person, is a matter of fundamental knowledge by faith.

The decisive factor in philosophy of value is not so much the *actual* delimitation of the separate steps in this hierarchy of intrinsic values, as the *principle of concreteness* of such values. And here I would emphasize another deduction from this principle: the concrete is not *rationally* comprehensible. We may be able to decide by reason at what points in the world we have a right to hypostatize intrinsic values; and, furthermore, we may, after such hypostatization, *deduce* such secondary values as follow from the existence of such intrinsic values. But the essence and individual form of these arch-values elude every attempt at rationalization. Intrinsic, personalistic Being is just what it is: with its inherent unique determination to constitute, from its particular locus, in its particular way, a microcosm of value; not resembling any other intrinsic value, not deducible from any general law, posited without foundation in its self-significence.

In the way in which the secondary values are now derived from these primary arch-values, we see once more the mutation which the doctrine of personalism has undergone by reason of its excursions into the philosophy of meaning. Formerly, all derivatives from the underivable person were opposed to the latter as "thing." This alternative is no longer adequate in the realm of values. Now, as ever, the intrinsic values are identified with personalistic ones, but the derivative values are not identical with "thing-values." "Thing-values" are such as have meaning only by virtue of *subservience* to the intrinsic values; they are externally related to these, and consequently are furthest removed from them in the scale of significance. But between the intrinsic and these purely extrinsic values we now discern those values which—without being ends in themselves—are *participant* in such ends, certain aspects or phases, external or internal tenderings of intrinsic values. Every self-significant *unitas multiplex* must project its unique arch-significance into everything that belongs to it, must radiate its essence thereinto; thus arises the significance of membership and of symbolism—which type of meaning is designated as *"radiative value."*

This mediating concept, as soon as it has dawned upon the mind, assimilates by immanent necessity large realms of value-philosophy. It has always appeared as something of a falsification to ask, apropos of certain value-phenomena, merely whether they were to be regarded as values in themselves, or as instrumental values for extraneous purposes; only at this point can we realize that besides being a "value in itself" or a "value for something else," there is also the possibility of being a "value *in* something else," and that this state of radiative value assigns the logical place of many value-phenomena.

Thereby the relation of Person/Thing is given a new meaning. The former attempt of personalism to overcome the opposition by the principle of teleomechanics was based as yet upon the assumption that the alternatives were exhaustive, and that the category of the "thing" could be directly derived from that of the "person." This is quite true, as long as we are concerned with the *explanation,* not with the *interpretation* of Being; for all scientific thinking, therefore, teleomechanics remains the proper methodological foundation. But for studies in the philosophy of meaning, personal value and thing-value now become the two poles of a *meaning-series.* The old duality liquidates itself into an infinite gradation, for the radiative values are sometimes, in their capacity membership or symbolism, very close to the intrinsic values, sometimes they effect an almost imperceptible transition to those thing-values which have merely external utilitarian and subservient significance. This liquidation of the personalistic fundamental alternative may be preparing theoretical developments of the system, whose scope and importance cannot as yet be estimated.

The most recent systems of value philosophy are *culturo-centrically oriented:* i.e., from the outset they derive realms of value (e.g., religion, art) and value-concepts (holiness, beauty, etc.) from the given cultural realms. Personalism proceeds quite differently: the category of value is metaphysically established. But just for that reason it can be made fruitful in another spirit for the cultural world. To this subject, too, the third volume contributes some lines of thought: as *radiative* values we may designate values of expression, spiritual values, historical values; as *instrumental* ones, the economic values. Now in considerable detail, now in allusions, the connections are here indicated which may lead to a personalistic foundation of the mental sciences.

For two scientific fields we will elaborate this a little further.

The syllabus of a *personalistic philosophy of history* is given in the form of theses (Volume III, Chap. 19). Genuine "history" is here distinguished equally from mere biological "development," and from the dialectic of a rationally comprehensible progress of ideas (Hegel) or the reduction to general values (Rickert). A fundamental distinction is made between historiousness and historicalness. "Historious" (historifying) applies to intrinsic personistic unities of super-individual character, which produce their own history, namely the way their self-determination is realized in the course of time. A characteristic of historiousness (as opposed to mere "development") is the "dually directed temporality;" for not only does the past function progressively, into the future, but every present reaches back into the past, relates the latter to the life-unity and *thereby is constantly remaking it.*

This thesis of the *"plasticity of the past"* is actually propounded in earnest, not in the sense that the whim of any historian remodels the picture of past events, but as the constant mutation of the historical past itself. For facts are "historical" only by reason of their relations to *historious* substrates, namely, "insofar as they are essentially significant for the self-realization of a historious substrate and connect, as accomplished Being, past and present of this substrate in dually directed motion."

Every case of historiousness of a substrate appears at once as subjective historical consciousness and as objective cultural formulation. These, however, vary their manifestations with the *degree* of historiousness. Nearest to mere biological development is latent historiousness; it is followed by "elementary historiousness" (mythical stage), "full historiousness" (really historical *Kultur*), and "reflective historiousness" (of the historical sciences).

As the historious personistic unities are subject to the hierarchy, *history is storeyed* (*ist Geschichte geschichtet*). Thus there is neither a mere parallelity of independent historical sequences (Splengler), nor a linear universal history (Hegel), but a system of life-sequences of subordinated and super-ordinated historical unities, each of which—as a self-realization of a unique personality—eludes rational deduction. Concomitantly, historical time receives new dimensions by this stratification.

Economic Philosophy (Volume III, Chap. 12). For national economy the "value"-category is a central concept. But its real object is merely that form of value which we call "instrumental," and which receives its significance only by deduction from personalistic intrinsic values. Comparability and quantification of values, too, which are basic to all economic thought, belong only to instrumental values, because they stand at the foot of the "series of meaning-contents" of values. The higher any value-bearing substrate ranks in this series, i.e., the more it possesses radiative value in addition to mere instrumental value, the less adequate to it is mere quantifying and comparative valuations. Thus economics, in order to appreciate the meaning and limit of its own value-theories, must know the place of specifically economic values in the scale of meaning-contents.

Finally, however, my contemplations return to the *single human personality*. Here philosophy of value becomes philosophy of the evaluating attitude. The possibility of the evaluting attitude of the ego toward the world is three-fold: in "ipsification" the ego posits itself alone as, so to speak, a punctiform intrinsic value, everything else as "thing"; (contemplative ipsification is subjectivism, active ipsification is egoism). In "de-ipsification" the ego repudiates its own intrinsic value as much as possible in favor of the intrinsic value of some sort of non-ego; (contemplative de-ipsification is objectifying knowledge, active de-ipsification is altruism and sacrifice). In *introception* both are simultaneously asserted by the person standing at the center of its value-cosmos: "the ego makes the acceptance of the non-ego-values a part of its own intrinsic value; yes, more: it really incarnates its intrinsic value only in that it makes the other values its own, and thus expands its own punctiformity to a microcosm." In the two-fold simultaneous acceptance of the ego-value and the non-ego-values lies another dialectic; in this attitude man approaches most nearly to the ultimate meaning of the world and of himself. We treat of the following fundamental types of introception: loving and understanding, aesthetic sensitivity and practical activity and creativity, finally worshipping. The essence of the *religious* attitude is "introception of the absolute (*verabsolutiert*) non-ego into the absolute ego." The absolute All-Person is for our objectifying knowledge a mere—howbeit necessary—limiting concept, but for introception, which loves, understands, sees artistically and adores, which accepts at once immediate dependence and immediate attachment, selfhood and membership, it is the living God.

Introception, however, this metaphysical attitude, is never simply and actually present, but just for this reason it is always seeking fulfillment. Thus it becomes normative for the formation of human life. The *ethical imperative* cannot be individualistically: Live out thy life, for this ipsification would ignore the objective values. It cannot be universalistically: Subject thyself to the general ethical law, for that would be de-ipsification, denial of the uniqueness and therewith of the unique task of every personality. It can only be: "Introcept!" or: *"Mould thy ego microcosmically into a personality, in that thou raisest all service to the non-ego-values to essential traits of thy individual intrinsic value."* Here the uniqueness of the ipsistic intrinsic value is wedded to the universality of superior value-norms; and the autonomy of the self proves itself in this, that it does not simply negate the heteronomy of those superior norms (compare Kant), but transfigures them into postulates and accepted norms of its own nature.

The imperative: "Introcept," is the *formal* basic norm for every morally responsible person; but the ethical demand becomes charged with meaning, concreted, only through the system of content-norms, which result for each individual from its *metaphysical situation.* For each human being, because he is the center of his value-cosmos, there is a unique norm-structure relative to him, which he can smelt into the fabric of his personal vocation only by his individual self-determination. Thus the content-giving ethical imperative is: "Live up to thy vocation!"

Hence personalistic ethics is by its form introception-ethics, by its individualized content, vocation-ethics.

The personalistic system, in that it begins as ontology and ends as axiosophy, shall meet the requirement which I, at the beginning of my systematic work, posited as a *demand* upon all philosophical world-contemplation as such:[49] "Whoever regards philosophical world-contemplation as worthy of his ambition, must admit that it can only consist in the synthesis, not the bifurcation of the universes, respectively, of cognition and value; and whoever seeks a philosophical world-view himself, must take this as the regulative principle of his researches: *to unite world-view and value.*"

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[49] See footnote 19.

CARL STUMPF*

"We scorn construction, love investigation, maintain a skeptical attitude towards the mechanism of a system . . . We are content at the end of a long life to have tapped various lines of scientific research which lead to the foundation of things; we are content to die on the way."—*W. Dilthey* (1865).

To the following "self-presentation"—the length of which I beg to excuse in view of the length of my scientific service—I consented only after some hesitation, when I realized on various occasions how difficult it was even for my scientific colleagues and pupils to find the thread unifying my much-ramified writings and to discover the roots of my scientific life-work. I hope this may be facilitated by the following.

I. BIOGRAPHY

I was born on Good Friday, April 21, 1848, in the little hamlet of Wiesentheid in Franconia, and on Easter Sunday I was baptized according to the Catholic rites. My parents were the County Court Physician Eugen Stumpf and Marie Stumpf, née Adelmann. Three brothers have been, and three sisters still are, my tried and true companions in joy and in sorrow. My parents, whose life and care were entirely devoted to the welfare of their children, were still living when I was called to Munich. My grandfather, Andreas Sebastian Stumpf, who died long before my birth, was a well-known Barvarian historian and a member of several academies. My father's two brothers also were active in science, and published works on statistics, biography, and forestry. My grandfather Adelmann, born in 1770, Court Physician in Gerolzhofen, had studied the French literature of the eighteenth century, as well as Kant and Schelling, whose works, with abstracts and notes, were found in his library. After his retirement, he came to live with us and taught me the fundamentals of Latin, and later on followed my progress with interest almost to the university. The Adelmann family, which came from Oldenburg to Fulda and Würzburg, numbered remarkably many doctors among its members. Five of these, among them three university professors, in Dorpat, Löwen, and Würzburg, I knew personally, four others only by name. Thus it may be that the love of medicine and natural science was in my blood. Both of my parents

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were musical, my father an excellent singer, my mother a good pianist. From them I inherited my love of music.

After a year in the Latin school at Kitzingen, I attended the "gymnasium" in Bamburg from 1859-1863, and the two following years at Aschaffenburg, where my father was transferred. This charming town became our second home.

As I was physically frail, but mentally intense and ambitious, religious, and over-conscientious, my mind developed faster than was really good for my nerves. But fortunately I could spend the first ten years of my life in the country, where not only a spacious yard, but also some farm-work stimulated physical activity. Other physical exercise also had an invigorating effect, such as gymnastics, swimming, and especially hiking with my brothers and sisters through beautiful Franconia, later from Aschaffenburg through the Rhineland and the mountains of central Germany, and still later through the length and breadth of the Tyrol and Switzerland. Walking and mountain-climbing in pleasant company seemed to me one of the most important aims of human existence—liberating and broadening the spirit—and the school semester, by contrast, a sort of purgatory preliminary to the heaven of vacation. Many young people in southern Germany probably feel much the same way. This passion for hiking has stayed with me even to my old age, and undoubtedly has helped me to attain the latter.

I do not remember the studies at the "gymnasium" with much pleasure, generally speaking. I made good progress, but only with considerable effort, as I was a year ahead of my age and did not have a good memory for history and geography. Of my teachers, I hold only two in grateful memory, especially the aged Hocheder in Aschaffenburg, senior professor of the graduating class, who was, incidentally, an impassioned astronomer, and through our study of the *Phaedon* first awakened my love of philosophy and of the divine Plato. I have ever remained, at heart, a disciple of Plato. The instruction in general was anything but inspiring, and even technically unsatisfactory. Mathematics especially was very poorly taught. I had no special talent in that line, but with a sound foundation in school I should probably have made greater progress in it.

There was, however, in the higher institutions of Franconia an excellent opportunity for musical education. Even in Kitzingen, from singing in the massbooks, I had learned the old-fashioned notes of the four-line system, and could soon sing at sight in any key.

At Bamberg we had a complete orchestra which met regularly at the free-standing Aula-building for practice under the direction of the excellent conductor Dietz. One could learn to play any instrument, free of charge. At the age of seven, I had commenced to study the violin, and during my student years had several opportunities to play in public. Besides this, I had learned without instruction to play five other instruments with more or less success. When we played or sang together at home, the leadership was left to me, and I formed the habit of hearing music analytically, i.e., by following the single voices or parts. Quite objectively speaking, I cannot understand how, without this ability, one can really appreciate in polyphonic music the beauty of the pattern, the weaving in and out of the individual voices, composition in the true sense. The copying of notes, which for reasons of economy I practiced assiduously, also aided me to gain an insight into the trade secrets of music, as it served Rousseau in a similar manner. In my tenth year I began to compose (my very first work was an oratorio, "The Walk to Emmaus," for three male voices), and during the last years of my course this developed into a dominating passion while I was studying the theory of harmony and counterpoint in the manuals of Silcher, Lobe, and Gottfried Weber. I composed quartets for strings and other pieces, but unfortunately inspiration did not always keep step with labored reflection. The only product of any originality was a scherzo in complete 5/4 time.

Thus, at the age of seventeen, I entered the university with more love of music than of erudition. In Würzburg I followed the proper Bavarian custom of attending lectures on general subjects. The course on aesthetics by Professor Urlich, the philologist, stimulated me to study the *Kritik der Urteilskraft* from my grandfather's library.

Thus Kant became another of my guiding lights in philosophy. During the second semester I decided to study jurisprudence, not from inclination, but in order to have a profession that would leave me some leisure for music. I diligently attended lectures on institutions and pandects, on the history of Roman and German law. But towards the end of this semester came the great change, by the addition of Franz Brentano to the faculty. Elsewhere I have already described the complete change which this man's appearance, his personality, his manner of thinking and teaching wrought in me. Everything else vanished before the great problems of philosophical and religious regeneration. Keen thinking had scarcely been in my line so far, and was rather irksome. Only through Brentano's iron discipline the craving for logical clearness and consistency became second nature. All emotional life had to submit now to the laws of reason. This was not to cripple it, but rather to direct it exclusively towards those aims that to us seemed the highest. I was ready to relinquish all worldly happiness for the realization of the ethical-religious ideas of Christianity in my fellow creatures and within myself. This was my condition of mind for four years.

Besides Brentano's lectures, I also took courses in natural science, as he considered both the substance and the methods of science important for philosophy. His dissertation, wherein he presented the thesis that the true philosophical method is none other than that for natural science, was and has ever remained a lodestar to me. In order to attain some practical knowledge along this line, I worked in the chemical laboratory, though with the final result that by some careless reaction I caused a small conflagration which might have spread over the whole building if the attendant had not come to the rescue. I never attained manual cleverness.

In my fifth semester, at Brentano's advice I went to Göttingen to study with Lotze, and to graduate there. How Lotze became my fatherly friend I have likewise mentioned elsewhere. His mental attitude had greater influence on me than Brentano really wished, although the fundamental epistemological lines were always those that Brentano had impressed upon my mind. Besides Lotze's lectures, I also took those of the physiologist, Wilhelm Weber. The latter, besides Brentano and Lotze, developed and formed my manner of scientific thinking. The modest old man, whose whole appearance in the lecture-room seemed at first awkward, even comical, had developed by the most intense mental effort a system of physics, which, better than any logical lecture, revealed to the student the methodology of inductive thinking. His course, which ran through two semesters, I took down in shorthand almost word for word. Ever since, physics has seemed to me the ideal inductive science. Friedrich Kohlrausch's research course introduced me to the technique of investigation. Today such preparation, at least for the psychologist, is a matter of course; but at that time a philosopher in the chemical and physical laboratory courses was a white raven—a *rara avis*.

My thesis I wrote with a special view to its logical form, and this may have been why Lotze, who at first maintained a skeptical attitude towards my subject and advised against it, in the end changed his mind. The procedure, which I had derived from Brentano, and indirectly from Aristotle, namely, to prepare for the final argument by a complete disjunction of possible opinions and a refutation of all but one, is found in many of my later writings. In preparation for the final examination, I read all the great philosophical classics, howbeit in a very cursory manner, and for my dissertation, the entire Platonic literature. Brentano's oral instruction and writings had naturally given me a pretty thorough grounding in Aristotle's teachings. How seriously the theory of ideas, which gave even Aristotle some troubles and which—*mutatis mutandis*—is repeated in modern German idealism, must have tormented me is shown by the cry of despair in my first disputation thesis, "Ideae nomen e metaphysica expellendum esse censeo." It probably did not please Lotze any too well. The same mood inspired also the initial question of a somewhat arrogant little essay during the time I was in Würzburg concerning the psychology of the present time: *Sind wir noch Idealisten?*

After my graduation in August, 1868, I returned to Würzburg to continue my philosophical studies with Brentano and at the same time to begin the study of theology. In the fall of 1869, I entered the ecclesiastical seminary in Würzburg where I was initiated into the liturgical ceremonies of the Church, the ascetic regulations, which I observed most conscientiously, and all the details of religious exercises. The theological lectures gave me no pleasure, except those of the genial old commentator Schegg, who had traveled through the Holy Land and could describe it most vividly. Besides, I studied most diligently Thomas Aquinas and other scholastics; and Hebrew, on account of the Bible. The fact that I now know only the first letter of the alphabet of this language is a striking example of the effect of disuse on memory.

Within the walls of the seminary, however, even in the spring of 1870, the second, still more fundamental regeneration overtook me, and again under Brentano's influence. The whole structure of the Catholic-Christian dogmatic theology and *Weltanschauung* crumbled to dust before my eyes. In terrible agony of soul I had to give up my chosen life work, my ideal. In July, I took off the black robe. I had not been ordained as yet, so there were no serious complications. But I had to find my way back to the world, and many favorable, as well as unfavorable, after-effects of this year I was to feel for a long time to come.

Soon, however, I decided to go to Göttingen to attain an instructorship in philosophy. Upon my entrance to the seminary, Lotze had written me a letter from which I have quoted his religious views in another article, the end of which, however, I shall add here:

> "The most important point I approach last. I am far from satisfied with the condition of the Protestant church and theology, and will let your criticisms pass, though I do not approve of them all. I suspect that you do not sanction everything that your church brings forth nowadays (its infallibity). The principle itself I cannot discuss with you, since I as well as you believe that the living faith is the only foundation for it. Your decision to become a priest I can accept only with deep respect for your conscientious conviction, and, although it destroys a cherished hope of mine, still I realize the full extent of the blessing that your strong spirit may carry with it in your calling; I realize this too well to think of opposing your decision in any way. Nevertheless, forgive me, who loves you so dearly, one urgent, rather serious, request: Do not now in your early youth, which you are still enjoying, take such a decisive step, an irrevocable one, too rashly! Everything else I leave to your good judgment, your consideration; but this one thing I beg of you!"

These words, revealing his respect for every individuality as well as his personal affection for me (he even intended to visit me during vacation in Aschaffenburg or Würzburg), I had treasured like a jewel in my heart, but realized now for the first time how right he had been with his "rather serious" warning. When he heard of my change of heart, he wrote me, in a similar vein, that he would consider it indelicate if he should offer to help me, in my inner struggle, with views which originated from entirely different starting-points; that I would fight it out all right by myself.

> "There is just one point that is troublesome, which I would mention here: Life is long, and yours, I hope, will be measured for you as long as for the most favored. Is it, then, necessary to settle all your doubts concerning the most important matters at once? Perhaps you are tormenting yourself too much by meditating incessantly about things which might be put aside for the time being, now that you have declined to make a binding decision; then, after your mind has had some rest and recreation, you can return to these problems with a greater calmness, impartiality, and receptiveness."

He approved my decision. During vacation I worked on a dissertation about mathematical axioms and at the end of October, 1870, I became instructor in Göttingen. I have never published this dissertation, as the non-Euclidian way of thinking to which Felix Klein had introduced me was, after all, a little beyond me.

The transition from the seclusion of the convent to the "city of the muses," which in the eighteenth century had produced the "philosophers for the world," and where even now, in spite of the War, sociability flourished, was extremely sudden and staggering. But my youth had enough elasticity to adapt itself, and I soon felt at home in the new milieu. Lotze's house was always open to me, as well as Baumann's and finally Henle's, at whose musical evenings I played the cello in the quartet. He was a man of the most genial humor and of great kindliness towards his friends. Even shortly before his death (1885) I received his charming chatty letters. His "Anthropological Lectures" are known for their keen psychological observations. During these years I met, besides the famous men of Göttingen, also those two veterans of psychophysics in Leipzig, E. H. Weber and Fechner, the former at the home of his brother Wilhelm, where he showed me on my own body various sensory fields, and the latter on a field-trip with Felix Klein. With Fechner I discussed the difficulties of atomism caused by the unity of consciousness, which he thought to solve by analogy with the unity of the concept. We also served him as subjects for his experiment with the golden section. The personality of these two great men, genuine scientific investigators, made a lasting impression on me. But there was also in Göttingen a fine cooperation among the numerous young minds. My closest friends were Felix Klein and the Scotchman, William Robertson Smith, who as a liberal Bible investigator later on suffered serious persecution in his native country. Klein, who even then felt within him the urge to organize, founded with me the "Eskimo," a society of young scientists, for the purpose of lectures and friendly intercourse, wherein I was to represent the philosophical part. Professors were excluded. The club is still alive—as far as I know—but with somewhat modified conditions.

I began my lectures with ancient philosophy, especially that of Aristotle, whom I studied intensively for a whole year. As my first more serious work, I attempted a critical history of the conception of substance, over which I racked my brain most awfully until I abandoned the problem, and, at Easter, 1872, took up the psychological theme of the origin of space conception. In the relation between color and extension I believed, and still believe, to find a striking example or analogue of the relation which metaphysics assumes to exist among the qualities of a substance. Thus the new problem was connected with my old work.

It progressed rapidly, and, in the fall of the same year, the book was printed. It appeared at a time rather propitious for my advancement, as there were vacancies in philosophy in five universities. In Vienna I was considered as a second choice, but at Würzburg, where Brentano and Lotze had spoken for me, an offer materialized, and in the fall of 1873 I was settled in my new position as professor.

It seemed great luck to find a position in a famous university so soon—especially for the sake of my parents. But there were also certain disadvantages: I had neither enough experience in life nor the necessary scientific maturity for the difficult position. As Brentano has resigned, and the aged Hoffmann, a follower of Baader, found scarcely any listeners, I had to represent, as it were, the whole Department of Philosophy; but with the courage of youth I gave, in turn, all the great philosophical subjects except ethics. The aftereffects of this over-exertion I was to feel for many a year.

In 1874, on a trip through Italy, I met—besides Bonatelli and Belotti—the leader of Italian philosophy, that remarkable man, Count Terenzio Mamiani, and his pupil, Luigi Ferri, both of whom asked me casually about the condition of German philosophy. In the same year I took a trip across the Channel with Smith and had an opportunity to fill out (in the British Museum) my knowledge of English philosophy, much of which Smith had already brought to my attention in connection with my book on space. Like Brentano, I delighted in this clear, logical—if not always profound—philosophizing, and the keen presentation of contrasts that we find in truly classic style in Mills's book on Hamilton. But Herbert Spencer's constructive manner always seemed tedious to me.

The first scholarly work I undertook was a history of the psychology of association, which was connected with my first-mentioned studies, but I gave it up as I had given up that of the conception of substance, and decided to devote myself henceforth to that field which, connecting my musical experiences and studies with the interests of psychology, seemed to me, personally, the most promising. In 1875 I commenced my work on *Tonpsychologie*. The excellent collection of acoustic devices at the Institute of Physics was placed at my unrestricted disposal through the kindness of my former teacher Kohlrausch of Göttingen. Besides, I frequently spent days in Hanau with the organ builder, Appunn, who had worked for Helmholtz, and we vied with each other in study and observation. I was well aware, of course, that such absorption in all the details of a field of sensation stood in sharp contrast to the general conception of the mission of the philosopher, although Fechner had been a famous example of this type. When I considered the hopeless condition, as it appeared, perhaps, in Überweg's review of recent philosophy—ever new systems without any connection with one another, each bent on originality, at least on a new terminology, none of them with any power of conviction—when I compared this with the evolution of physics, what a vast difference! Might it not be possible for a specialist in philosophy to work together with other specialists, at least in some particular field? If this were done by others in other fields, might there not result finally a beneficial relationship between philosophy and the single sciences?

Thus the time in Würzburg marks for me the beginning of a new line of work to which I have remained faithful to the present day, which, however, has made me an outsider to the great majority of my colleagues. My work of observation and experimentation has absorbed my time and strength even more than is the case with most experimental psychologists. Although I fully appreciate the saying of Aristotle that theory is the sweetest of all, I must confess that it was always a joy and a comfort to pass from theory to observation, from meditation to facts, from my writing-desk to the laboratory; and, thus, in the end, my writing-desk was neglected and has not produced a single textbook or compendium, which indeed ought to have been its first duty, even at the time when I was an instructor. However, I never intended to spend so much of my lifetime on acoustics and musical psychological studies as I did later on. I had counted on a few years. But it was, after all, not musical science but philosophy that always remained mistress of the house, who, it is true, granted most generously great privileges to her helpmate.

In this gay Frankish city, however, one did not live only to work. There was a large circle of friends and plenty of fun, but to talk about such matters would be quite out of place here. Among the older men, Kohlrausch and Wislicenus were my most intimate friends;

among the young scientists there was Erich Schmidt, who took my lectures on metaphysics, besides the buoyant archaeologist, Flasch, and the Romanist, Mall, a native of the Palatinate, who had absorbed the air of Berlin during the stirring sixties, a sort of Mephistophelian Merk, whose influence had a good deal to do with my withdrawal from Brentano's unconditional optimism. After five years I was thoroughly tired of a bachelor's life, and I realized that a certain attachment of the Göttingen period had taken deeper root than I had been ready to admit even to myself. Music, Beethoven's great wonderful Trio in B Major, had brought us together. Meanwhile, Miss Hermine Biedermann had taken a teaching position in Berlin. She followed the new call, and soon we were united for life. The great Trio in B Major, however, became our family trio.

In 1879 I received a call to Prague to succeed Volkmann. The faculty had thought at first of Otto Liebmann, but Brentano, who had been teaching in Vienna since 1874, had recommended me, without my knowledge, in order to gain in Austria a firmer hold for our theories. Under these circumstances I hesitated, but finally I accepted, partly because the strange romantic city on the Moldau appealed to my innate wanderlust, partly or indeed mainly because my influence in Würzburg, for local reasons, had greatly decreased during recent years. A philosopher who does not specialize in popular lectures can expect a large audience in Würzburg only if the students of theology attend his courses. This was the case during my first semester. But, as I in no way concealed my independent attitude toward the Church, the theological students gradually dropped my lectures almost entirely. A religious Protestant, like Külpe, is much more acceptable to the Catholic theological faculty than an heretical Catholic.

In the fall of 1879 my work in Prague commenced. The following year came Marty from Czernowitz, my best friend during my college days in Würzburg. The intercourse and professional cooperation with this man, remarkable for his keen mind and strength of character, whose studies in the philosophy of language led him deep into thought-psychology, was a great boon to me. It is, perhaps, not quite wise in assembling a faculty to maintain that the members of the philosophical department should hold different or even opposite views. If the point of view itself is not too one-sided, both students and teachers will gain decidedly by harmonious cooperation of like-minded leaders.

In Prague I had to give, every winter, a long course in practical philosophy—obligatory for the law students—which, so far, had concerned me very little. I at once worked out systematically and thoroughly a most comprehensive course, including philosophy of law and of the state. In this connection I picked up many loose threads of my brief experience as a law student, and became especially fascinated with problems of penal law. Later on, I gave, repeatedly, courses in practical philosophy and on the theory of voluntary action; the last time was in Berlin, in 1896.

The strenuous work of the first winter, together with family trouble and the unhygienic conditions of the city, seriously affected my health. However, in the second year I was able to resume my work on tone psychology, although the necessary apparatus was almost entirely lacking. To the investigation of extremely unmusical subjects, commenced in Würzburg, I now added the study of the theories of music of antiquity and of the Middle Ages and also the study of the ethnological literature of music—such as it was at that time. In 1883 the first volume of my *Tonpsychologie* appeared, which, in spite of long preparation, was, just like the book on space, finished only after it had gone to press, and shows the effects of this procedure.

Among my colleagues, Marty, Mach, and Hering were professionally closest to me. I never became personally intimate with Mach, in spite of my high esteem for the man, whereas I have maintained friendly relations with Hering all my life. These two men were the leaders of German rationalism at the University. During the struggle for our nationality, which rose to great intensity under the Taaffe ministry, I myself became a good German and learned to hold the Bohemian Germans in high esteem as a serious industrious branch of our people steeled by centuries of fighting for their national existence. The year 1882 brought to us our great joy, a visit from William James, who had liked my book on space, and with whom I soon found myself on terms of friendship. Later we met again in Munich and we kept up our correspondence to the end, though I could not follow him in his conversion to pragmatism. In his letters, published by his son, the genial, warm-hearted disposition of this brilliant man is particularly well revealed.

In the summer of 1884 I received a call to Halle to take Ulrici's place as a colleague of Haym and J. E. Erdmann. My longing for the German Fatherland had become so intense that I accepted the call with great rejoicing. In the quiet town of Halle I met G. Cantor, who was greatly interested in philosophy; and, since 1886, Husserl, recommended by Brentano, was first my student, later an instructor, and became intimately associated with me scientifically and as a friend; nothing here could interfere with my work, except the active social life, which I never could stand very well; but I made good progress with the second volume of the *Tonpsychologie*. That I had to make the fusion-experiments on the cathedral organ, instead of in a psychological institute, was no disadvantage, as there is no richer source of constant tone waves, of all possible shadings, than a good organ. On the other hand, I felt very keenly the lack of necessary apparatus, but I was able for the first time to make musical experiments with primitive subjects, i.e., on the Bellakula Indians and other tribes, who, through the efforts of Alfred Kirchhoff, honored the city with their visit.

In 1889 I was called to Munich as the successor of Prantl. Again I did not hesitate to accept, happy in the prospect to be nearer my old home; and in the fall of the same year I was settled in my beloved Munich. Here von Hertling, also a pupil of Brentano, was the exponent of Catholic philosophy. He was a loyal colleague, but on account of our diverging views we never became personally intimate. My dearest friend was the aesthetically minded philologist, Rudolph Schöll, who unfortunately died at an early age. For experimental psychology, and more especially for my acoustic studies, I could now gradually gather a collection of apparatus which was paid for from the faculty exchequer. This collection was kept partly in a closet in one of the corridors of the University, whence I took the instruments on Sundays to one of the lecture-rooms for observation and experiments, and partly in the upper story of the high tower, which still stands among the back-buildings of the University. The assistant of the Physical Institute had bought, for a song, a tuning-fork piano, which might have dated from the times of Chladnis; this he had taken apart, and he sold me the tuning-forks, a "continuous tone-series," with which I made many observations for the second volume of the *Tonpsychologie*. That is the way one had to manage in those days.

In Munich, as a member of the Academy, I wrote a number of academic treatises—hack-writing, in a sense, as one had to choose one's subject with some regard to the space-limits, into which philosophical subjects are less easily fitted than themes of history, philology, or natural science. Many of the lectures I gave in Berlin remained in manuscript, but the customary condensed tables of contents in the assembly reports I have added to the index of my writings, since they can at least suggest my views on the various subjects to any one who might be interested.

My severe criticism of a piece of work emanating from the Leipzig Institute involved me in a discussion with Wundt, which he, on his part, spiced with the most scathing invectives. That I was objectively right was proven by the fact that the results of the experiments in question—supposed to upset Fechner's law—were never and nowhere mentioned again, so far as I know, except in Wundt's textbook. However, I did not hesitate to express my opinion of the later acoustic work of the Leipzig school, nearly all of which I had to condemn; but I hope that I never overstepped the limits of objective criticism. Wundt's methods of procedure had been repellent to me even since his Heidelberg days, and continue to be so, although I admire his extraordinary breadth of vision and his literary productivity, even in his extreme old age.

I never imagined that I could leave Munich again, but, after five years, as in Prague and Halle, temptation approached me once more. Althoff tendered me an invitation to Berlin, where they wanted an experimental psychologist, when Zeller resigned, and Dilthey represented the historical approach. Although the call was a distinct honor, I had never felt any love for Berlin, and feared especially that there I should not be able to carry out my scientific life-work as I had planned it, so I declined. But, after a few weeks, I began to realize that Munich, after all, was not the right place to realize my ambitions. It was impossible to found an institute. I had appealed to the Minister of Education, who had always been most accommodating, for a yearly appropriation of five hundred marks for experimental psychology. His answer was that such a sum might be attainable, but that he would have to put the matter before the legislature, and there he might meet with the reproach that he was favoring materialism. Thereupon I declared that I should have to leave. Soon after this, however, Lipps was granted an endowed seminary, and later, Külpe a large institute. So the real reason for the Minister's attitude was probably quite a different matter, namely, my decided opposition to certain ecclesiastical wishes, shared by the court, in regard to the Academy.

Thus, at Easter of the year 1894, I went to Berlin, and now, after thirty years, I still believe that my decision was for the best. My fear that I might not be able to finish the *Tonpsychologie* and other greater works I had planned, unfortunately, proved well founded. But the psychological seminary, which started in three dark back rooms, developed into a large institute; and I have been able to pursue every kind of work, often fully, in every direction that interested me. Berlin's genius loci, the all-pervading spirit of work, had caught me. Inspirations came a-plenty, and there was no question, however remote, on which one could not find an expert opinion. Berlin was, moreover, musically the foremost city of the world, and Joachim, that noblest of performing artists whom I had known for some time as a friend, was still in his prime. All the great men with whom, during these many years, I came into closer touch officially, personally, and often socially, I cannot even name here. But I do want to mention that fact that I was able to associate personally with Helmholtz for at least one semester, and with Mommsen, for a decade; to maintain most cordial and harmonious relations with Dilthey, Paulsen, and their successors; and to renew my old friendship with Erich Schmidt and Kohlrausch. The personal intercourse among the colleagues of the University was kept up, in spite of long distances, not only by social life but also by the weekly faculty and academic meeting, and I considered it most fortunate that the large College of Arts and Sciences, in spite of its immense administrative burden, remained undivided. Through the many points of contact between psychology and modern thinking and living, I found that the great city harbored, besides men of sincere scientific interest and attitude, dangerous persons with questionable ambitions, who, under cover of art or science or even social welfare, pursued idle or commercial aims. This fact has often engendered disagreeable and time-consuming friction.

Since I feared not only the distraction from my own work, but also the danger of wholesale production for such a new scientific departure, it was my own wish that the experimental equipment and locality be started on a small scale. But soon the needs of the students required an extension which was now, of course, more difficult to obtain. In 1900 the seminar was turned into a much enlarged institute, but ever and again there were new requirements, requests, petitions. In 1920 we were given twenty-five rooms in the former imperial castle, whose management under the generally difficult circumstances caused me much trouble, until I was able to relegate it to younger hands. From this original institute there developed in the course of time four smaller establishments devoted to medicine, theory of music, and to military purposes; they are conducted by students. Much more active that I in the development of the equipment were my assistants, first Dr. Fr. Schumann, and, later, Dr. Rupp, the enthusiastic and expert constructor of apparatus.[1] These men also conducted the experimental courses, while I had charge of the theoretical meetings, in which we discussed psychological problems a propos of various recent treatises, and emphasized, in the spirit of Brentano, not only the need of psychological observation but also the necessity of logical thinking. I laid particular stress on these meetings because I regard the experimental method—at least of the external sort—by no means as the cure-all for psychology. For some time we were especially concerned with the theory of volition and questions of legal psychology, in the discussion of which certain men took part who later became prominent in the profession, such as Kantorowicz and Radbruch. This highly fertile field should be, I believe, investigated much more thoroughly by psychologists. The theory of volition was also the subject of several academic lectures, which were never published.

My studies in acoustics at Berlin, in which I was assisted, even during the first few years, by Abraham, Schaefer, Max Meyer, Pfungst, and later on by von Hornbostel, von Allesch, and many others, were initially of a purely physical nature, and were published in the *Annalen der Physik.* By testing musical sources for their overtones and by the production of absolutely simple tones by the interference method, we laid the foundation for all subsequent acoustic experiments at the Institute. These have been collected since 1898 in my *Beiträge,* of which the first volume, containing my *Konsonanztheorie,* had been intended for the nucleus of the third volume of the *Tonpsychologie,* but now had to be published by itself. Our acoustic equipment gradually reached a state of unusual completeness, but was suggested and developed entirely according to the requirements of the investigation; not a single piece served merely for demonstration.

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[1] Further details of the developments of the Institute up to 1910 will be found in Lenz's history of the University of Berlin, Volume 3, and in the annual chronicle of the University.

In 1896 von Shrenck-Notzing and I took charge of the preparations for the Third International Congress of Psychology in Munich, also of its direction. The attendance from all countries was enormous, and the resulting correspondence consumed a large part of my time. As my theme for the inaugural address I chose the vital question of the relation between mind and body. I endeavored to prevent hypnotic and occult phenomena from occupying the foreground, as had been the case in former sessions. The related departments were likewise represented by prominent investigators, as Hering, Flechsig, von Liszt, Pierre Janet, Richet, Forel, Flournoy, and Sidgwick. There was many a sharp conflict and spirited encounter, and, without doubt, much that was interesting and stimulating. Nevertheless, there has been no subsequent International Congress of Psychology in Germany since then, and it was considered more advantageous to discuss such moot questions in the domestic circle of the *Deutsche Gesellschaft für experimentelle Psychologie*, where foreigners also could take part.

With some phonographic records of a Siamese company performing in Berlin, I started, in 1900, the Archive for Phonograms, which was further developed by Abraham and von Hornbostel and later on conducted entirely by the latter.

At this time the work founded by Spitta, *Denkmäler deutscher Tonkunst*, and discontinued after his death in 1894, was reorganized by R. von Liliencron. I had been a member of the Commission since my coming to Berlin, and now, at the urgent request of Liliencron and Althoff, I consented to substitute for the deaf, eighty-year-old president, and kept his place until he died in 1912. The friendship with the venerable scholar, a nobleman in the true sense of the word, was a great privilege. For the rest, I thought of Mommsen's saying that in every commission there should be one member who knows nothing about the matter in question. Still, the merely formal direction of the discussions I could assume with an easy conscience and could increase, thereby, my knowledge of the old masters in a most desirable manner.

The same year I started, together with the principal, Dr. Kemsies, the Berlin *Gesellschaft für Kinderpsychologie*. By means of this organization I hoped to induce the teachers, especially of the intermediate schools, and also medical circles and educated parents, to take an active part in psychological studies and observations of the mental life of the child. I myself had repeatedly found these valuable in tone psychology, and I had kept a careful record of my own children. For several years this enterprise was very successful; during this time, among medical men especially, the famous child specialist, Dr. Heubner, took an active part. Two lectures of mine, later included in the collected lectures, were suggested by this work; the one concerning the peculiar speech development of a child has been especially noticed in the literature. It appeared, gradually, that the teachers were kept away by the pressing duties of their profession, perhaps partly, also, by their suspicions against the reform-threatening psychology. At that very time the work of applied psychology and school reform came so forcibly to the front that there was no room left for a society with pronounced theoretical aims. Other duties forced me to give up the leadership, and during the War the society quietly passed away.

Frequently I have had the opportunity to study prodigies. Thus, in the year 1897, the nerve specialist, Placzek, led me to examine a boy of four years, who had a most remarkable memory. Since his second year, he had been exhibited in scientific societies of different countries, even at the Berlin Panoptikum. As a consequence of my detailed report in the *Vossische Zeitung,* a prominent newspaper with the financial aid of some rich patrons, a governess was engaged to help the child through the most difficult years. In school the miraculous abnormality, being incompatible with a normal development, gradually wore off. Now he has become, to my great satisfaction, an efficient school principal. In 1903 I studied the early signs of musical talent in the child prodigy, Pepito Arriola, whom Richet had already exhibited at the Paris Congress. He became a noted pianist during his sojourn in America, but not a great composer, as Arthur Nikisch and I had hoped, from his achievements as a child. Among many others I also examined the young Hungarian, Hyiregyházy, about whom Révész wrote a whole book.

Such pedagogocial-didactic applications of psychology, arising in connection with child psychology and memory experiments, gave birth, at the beginning of this century, to applied psychology. In the Psychological Institute, Professor Rupp devoted himself to this new branch and now has a whole division set apart for it. I, personally, was not interested, but I aided its bold endeavors whenever the necessary precaution in execution was not overlooked.

In 1903 my interest was aroused by Krueger's investigations of combination tones on which he founded a new consonance theory, and I undertook an experimental investigation of this field, which, with some lengthy interruptions, kept me busy until 1909. That I should spend so much time and effort on a comparatively small and unimportant field of phenomena, to which I attribute a physiological rather than a psychological significance, might cause some surprise; but whoever reads the treatise will admit that here some questions of methodological principles had to be settled and that there were many special questions of fact which could be answered by the newly developed processes. Still, it is true here, as elsewhere, that if I had known beforehand how long this work would take, I should never have undertaken it.

The year 1903 brought a diversion towards which, for the sake of concentration, I ought to have been less susceptible. The engineer Cervenka of Prague had been induced by two Berlin investigators to demonstrate in the assembly hall of the University an alleged highly important phonographic invention, and the most distinguished personages as well as the entire faculty were invited. It was claimed that photographs of sound waves had been changed back into sound. We of the Psychological Institute, as well as the representatives of the gramaphone company, suspected that here on hallowed ground a bold deception had been perpetrated. I wrote a challenging, sarcastic article, and followed it up with a second one in collaboration with the physiologist, Engelmann. The work of exposure was made very difficult for us; but, finally, we produced conclusive proofs, and, thereafter, not a single word of the great invention was ever heard again. The affair had, however, some positive results. One was a revolution and a complete reorganization of the International Musical Society.

Shortly thereafter I was involved in another affair, more directly concerning psychology; it was the case of "clever Hans." In 1904, having just returned from a celebration of Kant's anniversary in Königsberg, after the lecture I was requested by a member of the Board of Education, to which Mr. von Osten had appealed, to investigate the matter, since the Board did not know just what attitude to assume in regard to the affair. That this was not a case of intentional deception was evident from the fact that the horse responded to the well-known African explorer, Mr. Schillings, just the same as to Mr. von Osten. Therefore an investigation seemed not out of place. I fully realized the extraordinary difficulties involved; the excitement aroused in the city and even in foreign countries by the daily reports of the strange case in the newspapers; the curiosity of the crowds which sought admission; the peculiarities of Mr. von Osten; the unfavorable locality; etc. The irresistible desire to determine the facts induced me to undertake the investigation, and we finally succeeded in revealing the facts, mainly by virtue of the keen eyes and iron patience of my assistant, Pfungst. In this case there were many interesting, more general results. Unintentionally, Mr. von Osten had confirmed by an experiment in a grand style Aristotle's theory of the absence of abstract reasoning in animals. For, if a method so carefully planned pedagogically as that which this former teacher of mathematics had used with untiring patience on his horse effects only the recognition of an unconscious movement of the head, then such failure must be due to the incapacity of the pupil. This solution, it is true, was not accepted everywhere. There appeared the horses from Elberfeld and the dog from Mannheim, with which professors of zoölogy and psychiatry actually entered into correspondence. In the *Journal of Animal Psychology* these men are still defending the presence of higher thought processes in animals. I had no desire for further investigation of such cases. Later, when the Academy of Sciences was enabled by the Sampson bequest to found on Teneriffe a station for anthropoids, where, at the suggestion of Professor Rothmann, anthropoid apes, coming directly from the jungles of our colonies, were to be studied systematically, I suggested Dr. Köhler for this investigation, and we all know how successful he was. Köhler did not attempt biologically useless stunts of calculation; his experiments were concerned with the important life-activities of the animals, and he proved that his chimpanzees in their use of tools and detours went far beyond the assumed limits of animal intelligence, and showed, in a certain sense, an "intelligent" behavior; only empirically intelligent, of course, not presupposing any general concepts, as arithmetic does.

In 1905 I was invited by the Kaiser Wilhelm Academie für Militärarzte (Pepinière) to give short annual lecture courses on whatever philosophical topics I chose, and I gladly seized this opportunity to interest the medical youth in philosophy and its history. It must have been about this time that the assistance of the Physiological Institute, together with those of the Psychological Institute and myself, founded the "Hirnrinde" to discuss common problems in a similar manner as had once upon a time been done in the old Göttingen "Eskimo." Soon some of the medical students joined us, among them Hugo Liepmann, who was chosen president. This society still exists and has proved very much worth while.

I was Rector of the University in 1907-1908. In my inaugural speech I expressed my conception of the present-day position of philosophy and its aims and problems. The position brought many interesting experiences, such as meeting the leading personalities of all circles; representing the University at scientific congresses; a conversation of forty-five minutes' length with the Emperor during my official call, when he did almost all the talking and expressed himself with astounding frankness. My daily occupation with curricular problems and students' affairs brought me great satisfaction, and, in the second semester, some unexpected excitement, through the struggle with the *Freie Studentenschaft,* which so far had always enjoyed my special favor. This union did not by any means include the entire number of non-incorporated students (*Finkenschaft*), but only a relatively small group who had assumed the right to fight for the interests and cultural aims of all non-incorporated students. But again and again they confused the representation of the *Finkenschaft* itself, and the small group of second- or third-semester students, or at least its self-appointed leaders, made demands which amounted to a co-regency. So the combat was on. There were vast general students' assemblies, in which radical politicians of the left wing, such as Breitscheid and von Gerlach, increased the excitement. They spoke of the murderer of academic liberty, of the rule of the Russian knout. I dissolved the *Freie Studentenschaft,* and with this discord the year ended. The Senate had always supported me. In the following semester the Board of Education permitted the reorganization of the student body with entirely new rulings to avoid the above-mentioned confusion. During the following years, a general student board was appointed, which constituted a real representation of the student body, while the *Freie Studentenschaft* continued their otherwise most laudable work. It is possible that a too strict insistence on minor points, which I might have overlooked, intensified the struggle which, however, had also burst forth elsewhere (Marburg, Halle). But sooner or later it had to be settled. That it fell to my lot I deeply regretted, for I loved the students, and the affair marred that otherwise splendid year. In the warning words of my second lecture as a Rector (on ethical skepticism) the echo of that episode mingles with a premonition of the trying time that was about to beset our Fatherland and was already predictable from unmistakable symptoms.

In 1909 the Berlin Philosophical Seminar, toward which Riehl and I had been working for some time, was established and splendidly organized by Erdmann. I belonged nominally to the directors but could take part only as advisor, and once by holding a seminar on Aristotle's metaphysics. I should have liked to establish here, too, a connection between psychology and philosophy, but the Institute did not permit of this. Occasionally Kant and Hume furnished the texts for philosophical seminars.

A pleasant interruption of the summer semester of 1909 was the request to represent the University at the Darwin anniversary in Cambridge. I had witnessed the rise and fall of Darwinism in its original form, but the idea of evolution had been bred in my very bones—as was the case with all my contemporaries; moreover, I felt such a profound admiration for the personality of this great investigator that I felt justified in accepting the mission. In my address, which was printed in the *Jahreschronik* of the University, I have expressed that admiration.

At the anniversary of the University of Berlin in 1910 the title of Doctor honoris was bestowed on me, and I gratefully appreciated this recognition of my efforts to establish a closer relationship between philosophy, psychology, and medicine. It was much less enjoyable that, in the course of time, I was forced to realize this relation as a patient and experimental subject by three dangerous abcesses of the ear, with two trepanations of the right temporal bone—and twice also as *casus rarissimus* of ophthalmology. But my ear passed its rigorous test *magna cum laude;* each time it completely recovered its hearing, and I could continue my investigations on vowels which I had started just before the last operation. My eye, unfortunately, just barely passed.

In 1914, at the Sixth Congress of Experimental Psychology, I reported the recent experiments on the theory of tone. On this occasion I offered a critical discussion of the radical vowel investigations by W. Köhler of the Berlin Institute, which had first been reported at the Fourth Congress in 1910. This led me to study the nature of vowels and of sounds of speech, in general, more thoroughly than had been done in the last paragraphs of the *Tonpsychologie.* The experimental results fascinated me to such an extent that I could not give up the investigation until this important field of phenomenology had been satisfactorily cleared up. Since the Institute was almost deserted during the first years of the War, I took advantage of the stillness of my surroundings for the most intense effort of the sense of hearing for my tone analyses. On the other hand, there were, of course, great difficulties and delays in the construction or repairing of apparatus. Furthermore, during the last years of the War the Institute was used by younger men for experiments in military psychotechnique (apparatus for measuring sound, etc.), and, naturally, my peaceful researches had to give way. Consequently they were not finished until about 1918.

During the War a call for collaboration went to the experimental psychologists of all the great countries involved in the struggle. As a representative of psychology in the Capitol, I took part in the national organization of this work. We did not attain, however, such a comprehensive and systematic cooperation as was attained in America.

In another enterprise eminently peaceful, although likewise suggested by the War, we have without doubt surpassed other nations. In 1915, at the suggestion of the school principal, Doegen, a large number of philologists, together with me, a musical scholar, undertook to make phonographic records of the native dialects, songs, and other musical productions of the prisoners-of-war, who were gathering from all corners of the earth, often from unknown and inaccessible regions. The Minister of Education appointed a commission of specialists drawn from all parts of Germany, who took technically excellent records in thirty-two prison camps, at the same time collecting the necessary material for the scientific study and classification of the records. Besides the grammophone records of the Commission, the Phonogram Archive had Dr. Schünemann make a large number of records with the more convenient Edison machine. The direction of the Commission was entrusted to me and took much time, consuming even my lecture time for a whole semester. But it meant much to me to observe, personally, the delivery and general bearing of these exotic singers, which certainly supplemented and enlivened my impression of the records. After the revolution, this entire collection was taken from the Commission without even a word of thanks and turned over to the State Library, where, in my opinion, no adequate provision has been made for its scientific upkeep.

Our old Phonogram Archive, which we had been collecting for twenty years and which consisted of about 10,000 records of inestimable value since those primitive tribes may die out or become civilized, were not taken over by the state at that time, and therefore were left without financial backing. After the state's attorneys discovered that ownership of the collection—to which we had really never given any thought—was vested in Mr. von Hornbostel and me, we put it at the disposal of the state with the understanding that the latter would attend to the upkeep and continuation of the collection. This condition was granted, and in 1923 the collection was turned over to the *Hochschule für Musik*. Unfortunately, on account of the general financial depression, which naturally affects, first of all, matters not pertaining to everyday life, the state cannot at present provide adequately for this purpose, so that our worries are by no means disposed of. It is some satisfaction, however, that, in spite of unfavorable times, we were able to found the *Sammelbände für vergleichende Musikwissenschaft* and thus have an opportunity to publish any articles in this line; and, furthermore, that the appointment of Messrs. Schünemann, Sachs, and von Hornbostel at Berlin makes this city by far the best place to carry on researches in that field.

At Easter, 1921, my official activity at the University was ended, on account of the new regulations concerning the age limit; but I continued my lectures until the summer of 1923. In Berlin, where the different branches of philosophy are represented by a large number of younger instructors, my lectures did not include general philosophy, but were confined practically to psychology, history of philosophy, and logic; in more recent years I have repeatedly given a course entitled *Weltanschauungsfragen,* in which I presented, as it were, a philosophical system. My lectures have taken much of my time until just a few years ago, since each semester certain especially unsatisfactory parts had to be recast. I was anxious to give a general view of the subject, to trace the history of philosophy up to the present time, but also to illustrate principles of scientific method by certain detailed expositions. I was not over-fond of lecturing, and often found it even an irksome task, interfering with the scientific research which was my chief concern and of course always led me more deeply into the subject-matter than the lectures—often, indeed, because of my special interests, along quite different lines of work. I have never, for instance, lectured on tone psychology or topics of musical research. Still, I recognized the marked advantage of combining teaching with scientific research for the very reason that it keeps in view the subject as a whole as well as in detail.

Since I had learned stenography in high school, I used to draw upon all sorts of shorthand memoranda in preparing my lectures. Only in recent years have my eyes forced me to dispense entirely with notes, and I must confess that consequently I take much more pleasure in my lectures, just because they are not literally "lectures" ("readings"), but speeches. I seem to be in closer and more vivid contact with my hearers. There is one disadvantage in using notes; by constant writing, one forms the habit of doing one's thinking while writing, and thus loses the art of speaking extemporaneously; still, the advantages are so great, especially for collecting material, making excerpts, and registering observations and experiments with all details, that, in general, I recommend it most warmly.

About 1907 I had resigned from the *Prüfungskommission für Oberlehrer* because the abominable preparation of the candidates, who were absorbed in their major subjects, disgusted me, and because the system of keeping records, especially of the pedagogy examinations, as it was practiced at Berlin, consumed too much time. The university examinations, too, at Berlin are a considerable burden on the faculty, for, in addition to every major subject in the arts and sciences, philosophy is required as a minor. But here the results were more satisfactory. It was my habit not to confine my questions to a single theme but rather to probe here and there until I struck bottom. Often I found that the candidate had developed a real interest in philosophy, not merely in the examination.

I belonged to the committee of the Academy for editing the works of Kant and Leibniz, and, after the death of Dilthey and later of Erdmann, I had to direct the work temporarily. I considered it lucky that during these years Kant's correspondence was finished—

the end of the long labor of editing his works—and an effective start was made on Leibnitz' works, which became possible quite contrary to our expectations. In the preface I recalled the enthusiastic words of Boutroux, the former director of the French Leibnitz Commission, which stand in sharp contrast to the present exclusion of Germany from international scientific enterprises, and I expressed the hope that the spirit of Leibnitz would sometime come again into its own. It gave me great pleasure that at the end I had to mention also my little native town of Wisentheid, where interesting Leibnitz documents had been found in the ducal Schöborn archives.

I cannot close this sketch of my life without mentioning that in 1921 I severed my connection with the Catholic Church. Although estranged for over fifty years, I had never formally withdrawn, being too well aware of the blessings our Church bestowed, nor had I any inclination to exchange my old confession of faith for any other. But the behavior of the officiating priest at the funeral of one of my brothers (he considered it necessary to apologize for standing at this grave, because the deceased, whose noble human qualities he later on felt constrained to praise duly, had not lived up to the regulations of the Church) induced me to take the decisive step. Though I am now non-denominational, as it were, I still confess myself with all my heart a disciple of Christianity as the religion of love and mercy—which needs no revaluation, but rather a higher appreciation—and I hope that in some time to come the different denominations will meet in this spirit, if not for a complete reunion, at least for a closer approach, a reconciliation.

II. Views and Researches

The following part of this paper has two aims: in the first place, to elucidate the purpose, methods, and results of my printed works, and, at the same time, to fill out, to supplement them, by connecting passages, so that the reader may find not disconnected fragments, but an integral whole through which the component parts, in turn, may be discerned and understood. If my presentation should seem dogmatic or even superficial, I hope that the reader will realize that this is not my usual procedure, and furthermore will find more detailed proofs in my writings.

First of all, let me say that the general tenor of all my views reflects the initial inspiration received from Brentano. To mention all points of agreement and dissent would take us too far afield. But it may be noted that the agreements pertain more often to the earlier than to the later form of his teaching.

Überweg (Austria) says, in the paragraph referring to Husserl, that I had started with Brentano, but later showed a closer approach to Husserl. That sounds as if Husserl's influence had changed my point of view in certain respects. This is, however, not the case. My deviations from Brentano's theories were the result of an internal, constant mental development. The pupils of Brentano naturally have many things in common in consequence of the same starting-point; many others, however, because of the necessity of changes, additions, and continuations simultaneously felt by those who proceed in the same direction.

1) *Definition of Philosophy.* However one may formulate the difference between mind and nature, everybody distinguished them in some manner. The philosopher, however, looks for what they have in common. Thus philosophy is primarily the science of things in general, or metaphysics, to which the gateway is epistemology. But that philosophers since olden times have generally regarded psychology as belonging to their proper field is due to the fact that psychic elements have been much more prominent than the physical in forming fundamental metaphysical conceptions. Therefore, it is to the point to define philosophy as the science of the most common laws of the psychical, and of the real, in general (or conversely). This is the only way in which we can justify the inclusion of logic, ethics, aesthetics, philosophy of law, pedagogy, and other branches in the domain of the philosophical sciences; the connecting link is always essentially psychology, which, therefore, must not forget—absorbed in experimental detail—the nobler phenomena of mental life which cannot be investigated in this manner and the great general questions.

2) *History of Philosophy.* Brentano's system of the four phases in which, so far, each of the three periods of philosophy since Thales has taken its course—a growing phase, wherein theoretical interests and empirical methods predominate, a decline caused by the smothering influence of some popular philosophy of life, followed by a skeptical and, finally, a mystical reaction—has always seemed to me a good key to the understanding of the development of philosophy, at least for antiquity and for modern times. In the Middle Ages the course

was greatly modified by the influence of the Church and the authorized faith. Historical similarities or analogies are not laws of nature. Of course, the scheme cannot be applied blindly to all details (where would sophistry come in, for instance?), and "decline" does not mean that during such stages profound, ingenious, and important achievements were entirely lacking. Finally, we must not forget that classifications from many other points of view are possible, although I consider the methodological the most important.

My first effect was devoted to the history of philosophy: my treatise on Plato's Idea of the Good and his conception of God. I tried to eliminate the contradiction between that philosopher's personal religious attitude and his philosophical system which Zeller had maintained by re-establishing Aristotle's conception of ideas as entities intrinsically different from concrete objects, and at the same time proving the identity of God with the Idea of the Good. This latter theory, which, incidentally, is shared by Zeller, is generally admitted today; concerning the right conception of Ideas the strife continues. I still consider the realistic conception correct, and Gomperz, Winderband, and Apelt agree with me, while transcendental evanescence seems ingenious, but unhistorical. My presentation, it is true, assumed too much of a closed system, and paid too little attention to the changes conditioned through Plato's course of development, especially the deviations in his last works for which philological methods have now given us a more complete understanding.

Among my later works, the two concerning the ancient theory of music (1897) contain many detailed discussions of passages in the text which have some importance for the history of philosophy, but have apparently not been noticed by my colleagues.

Two decades later, after much experimental work, I wrote a treatise on Spinoza, not because of any special sympathy with his philosophizing, but rather because I thought that I might say something new concerning one of his main points, the parallelism of the attributes. I believe I have demonstrated that his theory, both in form and in thought, is fundamentally different from modern psychophysical parallelism and is only an outflow of the old Aristotelian-scholastic theory of the parallelism of acts and contents of consciousness. The second study discusses the infinite number of the attributes and endeavors to elucidate the terse suggestions of the philosopher and to carry them out, hypothetically at least, on the basis of the theory of parallelism; and to explain how the author, in spite of the vast number of objective attributes which constitute substance, could maintain their unity. A third study was to discuss the "geometrical method," and find for the first propositions of the Ethics, and their proofs which Leibnitz justly condemned, the unconscious assumptions which made them seem formally necessary to Spinoza himself. Criticism so far has approached too much from the outside. The tasks of interpreting most clearly Spinoza's extreme conceptualistic realism and at the same time his dependence on scholasticism we recommend to those who delight in logical studies.

The greatest methodological achievement in philosophy since Descartes I find not in Kant or Hegel, but (with Brentano) rather in Locke and Leibnitz, and to these I would add Berkeley. Even though phenomenalism and the polemics against general conceptions really rest upon a misunderstanding, still we find mistakes of this kind even in the greatest thinkers; Berkeley's clear and precise presentation, however, and the energy of his thinking place him even above Locke, who excels him only in versatility. That Leibnitz far excels his predecessors no one will deny today. Among the immediate predecessors of Kant's *Kritik* I was especially fascinated by Tetens, whose *Philosophische Versuche* quite justly has been called the German counterpart of Locke's *Essay*. During my sojourn in Halle I suggested to Schlegtendahl and Störring their analysis of this work, and later I myself devoted a treatise to his theory of relations (*Psychol. u. Erkenntnistheorie,* Anhang 2). The spirit of unprejudiced and thorough research has probably never been so vitally effective in any other German philosopher before Lotze.

Kant's intellectual and ethical greatness is revealed above all in his re-establishing in full force the idea of necessity and its complement, the conception of duty. But, while he is still caught by one foot in the hypercriticism, he is already standing with the other in the speculative dogmatism of the subsequent period. Both tendencies, but especially the latter with its constructive mania, I cannot possibly consider worthy of imitation as the ideal form of philosophizing. Kant and the critical philosophy I have discussed repeatedly, and the post-Kantians have been taken up in my treatise *Wiedergeburt der Philosophie.* But whether we are already generally and definitely in a period of ascendency, I am inclined to doubt, as did Brentano in his last years. The numerous and varied beginnings, none of which rests upon each other, bear little resemblance to the systematic progress of true science. Even in psychology the disintegration has assumed dangerous forms; but here one may find some comfort, at least, in the Heraklitean dictum that struggle is the father of all things, since, after all, the foundation of established facts is steadily expanding.

The *Tafeln zur Geschichte der Philosophie*—with the third edition of which Menzer assisted—were designed for teaching purposes rather than as aids to historical research. They originated in Munich, when Prince Friedrich Karl of Hessia attended my lectures on logic, and I instructed him in the history of philosophy during our walks in the English garden. The scheme of lines, etc., pleased my colleagues none too well; but let it not be forgotten that it was designed for beginners.

3) *Epistemology and Logic.* These two disciplines are distinguished by the fact that epistemology pertains to the theoretical, and logic to the practical, the directions for testing and discovering the cognitions. Psychology, which treats of the processes of thinking and knowing, as such, besides other processes, is not basic for either of the two, but at the same time neither of them can dispense with it. By Kant's fundamental theses I demonstrated how the neglect of psychology always tells, but condemned also the psychologistic attempt to deduce the criteria of truth from the mechanism of psychic functions.

a) *The origin of fundamental concepts (categories).* To consider these as a priori would mean simply to cut the Gordian knot. We must try again and again to discover the original phenomena which form the foundation of their perception. Thus in regard to the thing-concept, or notion of substance, we may point out that in certain apperceptions we actually and directly perceive the close interpenetration of parts of a whole. Even in every sensory feeling the "attributes," quality, intensity, extension, etc., form not a sum, but a whole, the parts of which, in fact, are only subsequent abstractions. In the realm of psychic functions, intellectual and emotional functions, and indeed all simultaneous states of consciousness, are intimately connected (unity of consciousness) and are directly perceived as a unity. Hume's principle of research was, therefore, not incorrect, but he did not observe carefully enough, or he could not have defined substance as a bundle, but as a unity of qualities or conditions.

With the notion of cause he also stopped too soon. There are actually occurrences in which we can perceive not only a sequence but an inner nexus. Whoever follows closely a train of thought is in a certain fundamental mood (interest) which is causal, and we are aware of it as such; it conditions the retention of ideas and everything connected with these, their comparison, combination, etc. The fact is not that we become interested in something and that *then*, after our interest has passed, its effects appear, just as in nature effect follows cause; here we are dealing with an immanent and permanent causality, which is observable in itself. In the case of natural phenomena the idea of cause is merely a matter of transference, and this, although unavoidable, is of no use to the investigator who is interested only in the strictly lawful sequence of events.

The conception of necessity or lawfulness[2] may be conceived in its full force by realizing the content (expressed fact) of a priori, self-evident judgments like logical axioms and purely deductive propositions.[3] This conception, again, is transferred to nature.

Of course the conception of truth is also rooted in the realm of judgment. That is true which is convincing to us, either directly or indirectly; and that is false the opposite of which convinces us, either directly or indirectly. One can also say that truth (or untruth) is that quality of the contents of consciousness by virtue of which they compel appreciation (or aversion) by purely objective standards. Here everything depends on the conception of self-evidence, which one might fairly call the fundamental conception of Brentano. Just what this means we must experience through such self-evident judgments as 2 x 2 = 4; it cannot be further reduced or defined. Self-evidence and truth are correlative conceptions; the former is, so to speak, the subjective aspect of truth, whereas truth itself is something objective, i.e., independent of the individual act of consciousness, a function of that which is conceived, not of the conceiving subject. All positivistic theories of truth, pragmatism not excepted, move in a circle. Only as maxims of thought, economy, and usefulness are they still to be encouraged.

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[2] In the "apodeictic judgments" traditional logic jumbles together four conceptions which are by no means always identical: necessity, certainty, self-evidence, exactness (Brentano).

[3] To the axioms belong also those expressions which state the connection between premises and conclusion of a compelling syllogism—"normative axioms"—which one cannot deduce from experience without incurring a vicious circle.

Actuality or reality means effectiveness. Therefore, the conditions of our own mind are first of all given to us as real. For here, as I said above, we have a direct experience of causality. If we were not internally active, we should have no consciousness of reality. In the second place, we recognize as real "outside" objects (psychic as well as physical) in so far as we observe their effect on us. Whoever calls divinity the "most real being" conceives it simultaneously as the original cause. General laws, on the other hand, are true but not real because they are not effective.

b) *The means of knowledge.* We recognize a priori, by pure reason, laws deduced from bare concept and from self-evident propositions. Such insight involves no determinations of fact, wherefore it is most fittingly expressed in hypothetical propositions. In the case of mathematics, which is immediately relevant here, the a priori character of its truths is to be maintained even at the present time. If there are three geometries, each according to some assumed curvature of space (viz., of spacial forms), then each one is a priori, in its own right, and only its applicability to objective space is a matter of experience.

A priori cognitions issue not only from mathematical notions, but from any conception whatever, and such cognitions may add to our knowledge. The mere conception of two tones includes their conditions as to pitch, strength, time, duration, etc., which may be asserted of this or of any other similar pair of tones. The mere conception of three tones of different pitch implies a definite relational order according to which one must be placed between the other two. The conception of a tone series arranged according to pitch contains the possibility of its continuation *ad infinitum,* which obviously cannot be proved by experience (see *Tonpsychologie*).

Such propositions, however, are not strictly synthetic, since they are understood not only by means of our concepts but about them, if the relations (conditions) are counted as part of the subject-matter. At any rate, we must ask how such analytical judgments as extensions of knowledge are possible. To answer this question, we need to find, among other data, the simplest and most general directly perceptible relations for this process and a theory of their apprehension. We have the beginning of such a general theory of relations, but it is still in need of verification and elaboration. The a priori judgments themselves cannot thereby become more convincing, but their epistemological structure and significance may become more comprehensible.

Facts as well as laws are recognized (experienced) a posteriori. The instantaneously given sensory contents and our own psychic functions are directly experienced, whereas their implications are experienced indirectly. The conclusions concerning an outside world independent from consciousness, and concerning the laws controlling it, have the form of probable inductions. The only way in which we can subordinate the phenomena of the mind to definite laws, such as warrant predictions, is by assuming an outside world, strictly subordinate to causal law, in which our bodies with their sensory and motor organs and other more or less similar psychophysical substances exist as parts of the whole. In place of this vast hypothesis, which includes an assumption of the validity of the causal law,[4] there seem to be two other possibilities: first, to assume single cosmic power (Berkeley), and, secondly, to posit an unconscious "productive power of imagination" within us (Fichte). But if one attempts seriously to develop these theories, they merge into that of the outer world. For, in order to derive explanations and predictions, one must attribute to the supposed agent as many parts as elementary particles of matter one has to assume for the other theory, and must also suppose the same laws to hold among these parts.

For the naïve, unscientific consciousness, of course, the belief in the outside world is no hypothesis and no product of reflection, but is connected instinctively with the sensuous phenomena. But *that* outside world is immeasurably different from the scientific universe.

The enormous significance of mathematical probability for the formation of hypotheses, which differs from the "philosophical" only in degree, was also recognized and emphasized by Brentano. But, since it has been repeatedly claimed that application of the conception of probability already includes presuppositions concerning the outside world and the laws of causality, I have devoted a special monograph to this question, and believe I have demonstrated that this is not the case. So-called probability a posteriori, too, as it results from the law of large numbers, does not include any such presupposition, and it is superfluous to look for a physical mechanism which forces events to yield to this law. The principle of objective "leeways," as von Kries employs it, taken in a wide sense (i.e., as including not merely spacial or temporal but also logical leeways, or disjunctions) seems to me to lead to the same conception. The calculation of probability is, therefore, purely a priori, deduced from the mere concept of probability. In logic it has not as yet been duly recognized. It is indispensable in the development of a comprehensible theory of induction. At the same time, the absolute impossibility of popular empiricism becomes apparent; for according to this view every inductive conclusion is based not only on facts but also on an a priori foundation. We can, therefore, agree with Kant not only in maintaining the conception of absolute necessity but also in assuming that nature is a product of the intellect, though indeed not quite in the sense or according to the principles of the *Kritik der Urteilskraft.*

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[4] Brentano deduced the law of causality in its most general form (no change without cause) a priori, but at the same time made use of the laws of probability. I have my doubts concerning this "immanent induction." The probability of the law according to this conception (which is essentially that of Helmholtz) becomes immeasurably great and can be considered equivalent to absolute certainly.

The laws derived from experience are not, however, exclusively causal laws. We must also distinguish empirical laws of structure or substance. In both cases we have abbreviated procedures in which major terms are assumed as sufficiently well established. In the former case we thus assume the general law of causality, in the latter, such regularities as chemistry, for instance, has established with regard to the co-existence of certain properties.

Concerning my own attitude toward some of the principles of logic, I will say the following: I have always maintained Brentano's sharp distinction of judgment from mere conception, but the treatment of all judgments or assertions as existential judgments and the consequent revolution of the syllogistic theory I did not accept, later on, mainly because I, like Meinong, could not conceive of universal affirmative judgments as negations.

The conception of *Schverhalte,* the "state of affairs," which is being more and more widely used (Selz, Külpe, and others), was introduced by Brentano, who was fully aware of its import. I have merely replaced his term, "content of judgment," by that which is current at present, and which, in fact, I used for the first time in my lecture on logic in Halle in 1888.

To the significance of fictions for scientific research I have always devoted a special paragraph in my logic, but have never treated them as more than a sort of scaffolding which is removed after use.

The old question of the most fitting division of the sciences I have discussed in a special article, not on account of unimportant points of form, but because of relevant factual researches in epistemology. I was especially eager to re-establish the old distinction between natural science and mental science, which is based on differences of subject-matter. I am pleased to find Becher, in his exhaustive work, siding with me.

c) *Philosophy of nature.* The admirable development of physics and chemistry, which form the most general foundations of our conceptions of nature, has always followed the above-mentioned course. Sensory phenomena always were and still are their starting-points, but their subject-matter has become more and more the objective world. They approach it by way of hypotheses which most daringly draw even the objective nature of space and time into its domain. That these cannot be really as they seem to us, even the most obvious analysis reveals. Space I should define as that property of the concrete world which enables us to take measurements of a geometric type, and time as that which allows of change and of the commensurability of changes. It must be admitted that change itself cannot be defined without time; the two concepts simply are correlative. The concept of objective time contains no notion of past, present, or future. This is a noteworthy fact which enables us to treat time as a fourth dimension of space in mathematical physics; this treatment, in my opinion, is simply a mathematical device in which the special character of time in relation to the other three dimensions finds expression in the formula itself.

We need not elaborate upon the fact that the transition from the mechanical to the electromagnetic conception of nature falls within the methodological domain we have outlined. The hypothesis of an external world has not suffered any restriction of its explanatory powers. Any assumption is physically useable if it is free from contradiction and allows of quantitative predictions by which it can be tested. The common-sense conception of spatial movements is the most obvious one to attempt, but it holds no specially privileged position.

The transition from action at a distance to contiguous causation, however, was epistemologically inescapable. I do not know how physical causality could be unambiguously expressed, except as follows: "If, between two contiguous substances, there exist definite combinations of conditions, then in both occurs a change in which the new conditions on each side are connected with the old ones on the other side, and every change depends upon the occurrence of such combinations of conditions." (This formula is easily expanded so as to apply also to psychophysical interaction.) This means that all action is really interaction, but also that there is no direct interaction of everything with everything, but that only contiguous substances can interact with each other. Thus atoms or electrons, without which physics and chemistry are unthinkable at present, cannot affect one another in empty space, but only through the mediation of some ether, which I consider, therefore, an indispensable postulate of atomism.

The introduction of the notion of Gestalt into physics, such as W. Köhler demands in his ingenious book on physical forms, seems to meet with certain difficulties from this point of view, for the law of interaction will always compel the physicist to pursue the course of an effect from particle to particle, whereas the psychologist in describing the facts of consciousness may emphasize the priority of the whole over the parts.

The difference between the living and the non-living seems to me to lie in the immensely complicated structure of even the simplest organisms, or germs. The complicated mechanical conditions under which the physico-chemical forces work, if properly analyzed, will probably suffice to explain (except perhaps for certain psychical reactions) the processes of nutrition and reproduction. Science cannot admit the existence of forces which act now in a certain way, and now in another, perhaps the very opposite, as it was assumed formerly with the old *Life Force,* von Hartmann's *Unconsciousness,* and Pauly's psychovitalistic factors. Neither do terms like entelechy or dominants tend to improve matters. On the other hand, it seems not impossible, but in fact quite plausible, that the well-known conscious psychic conditions such as pleasure and pain, emotions, and volitions act as stimuli for nervous processes. Psychovitalism in this empirically controllable form would probably have been admitted even by Lotze, who was the keenest opponent of the old *Life Force.* E. Becher's interesting evolutionary "principle of exploitation," for instance, probably rests on some such foundation.

The philosopher, however, is more interested in the still more general problem of teleology than in vitalism. The innumerable intricately arranged particles, which even a unicellular creature presents (its environment must also be considered, since organisms without a definite inorganic setting are unthinkable), effect united life-sustaining processes. The problem is, as Galiani truly says, one of mathematical probability. Every such complex is just one particular case among innumerable other possibilities, ateleological, senseless arrangements of the same atoms, which in themselves are just as possible. It is, therefore, a priori most improbable, and although it is empirically given, it calls for an hypothesis to dispel the improbability. The doctrine of evolution solves many riddles, just this one it leaves unsolved. For, if the present forms have developed in uninterrupted regular causality from certain initial conditions, then these initial conditions, however simple, again must be particular cases of the same degree, since to each of the now imaginable ateological combination belongs a different initial condition from which through the agency of the same natural forces, it necessarily had to develop. The problem of purpose, therefore, is only pushed back by the doctrine of evolution. This holds also in case the world process has gone on since eternity, for the mathematical ratio of the actual cases to the other possible ones remains the same. Some ordering principle is, therefore, logically necessary. If we call this principle an Intelligence permeating the world, we are already using an expression which belongs to a special realm, though it be the highest realm known to us. But if we realize the inadequacy of any conception and the impenetrable mystery of this primeval being, then this last step is perfectly in keeping with the spirit of scientific thinking.

4) *Psychology and the Philosophy of the Mind.* The separation of natural sciences and mental science is based on the fundamental differences of sense data and psychic functions, or of the respective contents of external (sensuous) and internal (psychological) perception. Phenomena and functions are directly presented to us in closest connection, but they are essentially different. Observation of the functions is the foundation of the mental sciences, which, however, are no more tied to their point of departure than are the natural sciences. Just as the latter proceed to the construction of the material outer world, so the former seek to understand the nature of psychic forces in general and the resulting actions and phenomena in terms of that inner life which alone is given to our observation. Psychology occupies the same place among the mental sciences as physics among the sciences of nature.

The investigation of sensory phenomena as such, which at the present time occupies such an important place, is not really psychology but simply phenomenology, a kind of prescience equally pursued by physicists, physiologists, and psychologists. Psychologists especially have taken it up because it offered a chance for exact experimental investigation and an opportunity to test the laws governing the psychic functions involved. I also have devoted most of my time to phenomenological preparatory work, but my real aim has always been to understand the functions.

a) *On phenomenology.* The statement that there are no simple sensations (phenomena) seems to me a decided exaggeration. We cannot observe tones without observing them, but this need not necessarily change them. According to all that we know about attention, it enhances its objects and favors their apprehension. Therefore, I see no cause for the barren skepticism of that popular objection, just as I cannot agree with the ambiguous statement of the "relativity" of sensations. Still, in my *Tonpsychologie,* I took my departure not from sensations but from "sensory judgments" prefaced by an investigation of the conditions of reliability, because sensations are given to us merely as contents of apperceptions which may be false or unreliable. Experimental psychophysics thus becomes a quantitative science of judgments. Among the sensory judgments, I distinguished the direct and indirect, and was opposed to the mania for introducing everywhere indirect criteria which are merely side-impressions. Further, I distinguished judgments of sensations and of sensory distances. Another thesis which I did and still do maintain is that relations among the sensations can be directly perceived in and by the sensations themselves. We cannot hear the relation between two tones, but we can notice it, and to notice is to perceive.

One of the main questions of phenomenology, it seems to me, is that of the attributes (fundamental qualities) of the sensations. Even in my book on space, the very center of my argument is the conception of the "psychological parts," i.e., of the dependent or partial contents which cannot be represented separately, because of their very nature, but can only represent independent modes of change in the total sensation. Husserl has developed these observations further in the conceptual direction. I discussed this also in my treatise on the attributes of visual sensation, but dropped the term "psychological parts" as inappropriate. In this paper I tried to preserve for the visual sensations the attribute of intensity, which is generally denied them at present. Quality, brightness, intensity, and extension seem to be inherent in all sensations, although in different degrees. In another main issue which was raised by Aristotle, namely, the question as to the unity or complexity of simultaneous and coincident impressions upon the same sense, I decided for complexity in the case of tone, and unity in the case of color; and rejecting all forced analogies, I have insisted upon the essential differences between these two senses with respect to their proper laws.

In the realm of tones, we must, first of all, determine the properties of simple tones, i.e., of those which are produced by vibrations of the sinus, since these, according to our experience, cannot by practice or attention be analyzed subjectively or dissected into parts, and, therefore, promise best for constant results. For their unfailing production I introduced the destruction of the overtones by interference tubes, demonstrating also that a sounding body responds only to a tone of approximately the same pitch and not to any fraction of it, as many physicists, following Wheatstone, formerly taught, and even Wundt tried to demonstrate by special experiments. In this way we gained a convenient device for analyzing tones or sounds, and it was found that tone sources, considered so far as simple, were still quite complex. In consequence of these developments, Rudolf König's famous series of observations with electromagnetic forks and the wave-siren, for instance, lost their point, which was directed against Helmholtz.

My views on the fundamental qualities of simple tones have changed since the *Tonpsychologie,* as I now recognize the "musical quality" recurring from one octave to the other, besides the "pitch," which simply runs parallel with the vibration figures, i.e., I accept the former as an equally original element in the individual development. This quality I discussed in detail in the *Tonpsychologie,* believing at that time that it could be treated as a by-product of the fusion of the octave tones, though of course, I have always recognized it as a fact.

The differences of fusion, which are now generally admitted in psychology, are also an old inheritance. They were known in part even to the Greek theorists. But even before I was aware of this fact, I discovered them at the piano during my sojourn in Prague, and later proved them by statistical evidence obtained from unity judgments of unmusical subjects. The differences appearing here in the figures of the unity judgments have afterwards been confirmed again and again.

Because of the importance of these differences for the theory of consonance, I was interested also in cases where they do not occur, namely, at the highest pitch and in the shortest tone impressions. This led me to my studies on the determination of the vibration-rate of very high tones through their difference-tones. This method showed that Appunn's tuning-fork series, then in general use, were marked with an absurdly high pitch. In the case of extremely short tone-impressions, it was shown that, instead of the musical intervals, only the distances were judged. Maltzew later obtained similar results for very high, hyper-musical pitch.

The fundamental phenomenon of music, namely, consonance, I defined in terms of fusion, and believe to have demonstrated at least the inadequacy of other definitions, including that of Helmholtz, and the falsity of the dualistic theories of consonance of Riemann and von Öttingen. I distinguished, however, between consonance and concordance, of which the latter is not a purely sensory quality of tones but depends on the introduction of consonant triads as the basic elements of our system of music. The rational motive for the construction of triads seemed to me to lie in finding, within the octave, the greatest number of tones consonant among themselves. This yields the division of chords into concords and discords and the foundation of the entire classical harmonies.

My views on the definability of consonance in terms of fusion have changed since then. I believe that we can recognize such elementary relations even in successive tones but that this fact can be explained only physiologically, not psychologically. Fusion, however, and consonance of simultaneous tones, now appear to me as consequences and not as causes of the relation. But the differences of fusion maintain, nevertheless, their great significance for the musical hearing and for the emotional effect of the intervals.

Massbestimmungen über die Reinheit konsonanter Intervalle was a study of "musical ear" performed partly on myself and partly on others. We determined certain deviations from the physically pure pitch, which appear to be based neither on the well-tempered nor on the Pythagorean pitch, but upon powerful, aesthetic motives and are most clearly marked in unusually musical people. Most striking was the constant elevation of the ascending octave in most simple tones by members of the Conservatory of Music, most of all by Joachim. In playing double tones on the violin, of course, it remains pure.

The article on subjective tones and double hearing compares observations made on myself in this field with entoptic phenomena in other fields which had so far been neglected. How subjective tones are to be fitted into the theory of hearing is still unknown; for this very reason an exact description of the circumstances of their occurrence seemed desirable, and I had only too much opportunity to collect material for it. The rare phenomenon of double hearing came to me as a sort of compensation after the operation of piercing my left tympanum.

In the tone-tables the formulae for calculation of intervals have a fair claim to more than immediate interest, as they serve for correct computations and predictions quite apart from the particular ratios presented, a fact which has some significance even for metaphysics.

The main outline of the purely physical treatise concerning compound wave-forms dates from the Würzburg period, when I still entertained doubts of Helmholtz's alleged analysis by means of the cochlea, wherefore the qualities of compound vibrations, as such, seemed important to me. But the very fact that the natural classes of these vibration forms do not appear at all in the phenomena of sound themselves is new evidence for Helmholtz's hypothesis. Many questions discussed in this connection, as the definition of the period in such wave forms, have since interested even physicists.

In the monograph on combination-tones it seemed important to describe as fully as possible the phenomena and laws of this most difficult subject, where only well-trained observers and co-observers can be used as subjects. The derivation of these tones from the properties of the membranous parts of the organ of hearing is now a task for physiologists.

I made many observations on the subject of beats (inter-tone, etc.). The fact that these may sometimes be obviated, by holding one of the tuning forks to one ear and the other fork to the other ear, whereas the discord remains, gave me in 1875 the first decisive proof against Helmholtz's theory of consonance. In other connections, too, I have frequently found the phenomena of dichotomous hearing instructive.

Contrary to the common theory of non-spatial property of tone sensations, I claimed place-criteria for both ears and differences of volume for high and low tones. The possibility of locating correctly in a few minutes—without moving the head—up to ten tones simultaneously heard (Baley) can be explained only by such immanent place-criteria. Von Hornbostel and Wertheimer are known to have made further surprising discoveries concerning the power of localization through the ear; the former has now extended his investigation to the acoustic perception of distance.

The analysis of vowels, sounds of speech in general, and the synthesis of vowels based upon these, constituted the subject-matter of my last experimental investigation, wherein extensive interference tests played a leading rôle. I postulated three initial conditions for the synthesis: a large number of prefectly simple tones, a delicate and constant regulation of the volume of each tone, and a guarantee of the naturalness of the vowels, obtained by unconscious tests. The results have been reported in several articles, and a book containing all of them is almost completed. For general phenomenology those views are particularly relevant which deal with the so-called "complex qualities," and are the result of all these observations. My experiments proved Helmholtz's much-discussed foundation of the theory of vowels to be correct. For most of the consonants, too, the pitch could be determined, and analysis was possible up to a certain point. Furthermore, the same methods of analysis and synthesis could be applied to musical instruments. The results of the experiments with sounds of speech have not only been included in textbooks of physiology, but have also been applied by aurists, and by telephone and radio experts, who thereby have confirmed them.

The laws governing the relation of the sensations to outside stimuli, namely, the law of specific energies and Fechner's law, also figure in my work. I believe that the difficulty of conception of Fechner's law may be solved by its interpretation for distances of sensation (a viewpoint reached independently by Delboeuf, Hering, Ebbinghaus, and myself), and by the fact that in regard to pitch a striking confirmation of it, or analogy with it, was found in the Asiatic musical scales with equal intervals (Siam and Java), which depend not on tonal relations but on judgments of distance. This formulation is, of course, not intended as an explanation, but only as a psychologically correct expression of the law. The physiological derivation which is currently accepted I consider correct, at least in regard to intensities.

I also count space among the attributes of phenomena. This view, which means that color is impossible without extension just as extension is impossible without some quality, that, therefore, even the very first visual sensations must somehow appear spatial (nativism) has almost completely replaced the empiricism of Lotze's time. Muscular sensations, which had been identified with spatial ideas or were considered at least as their indispensable conditions, must be content to play a humbler part. Only the third dimension, which obviously is not so well represented in our intuition, is still struggling. The three syllogisms of my book on space I can, indeed, no longer approve in their given form; they were really only meant to be descriptions of that which we find in our ideas of space, in the way of necessary properties of depth. Some other things in this part of the book no longer hold true. But I should like to point out that I have never conceived of spatial sensations as depending directly, and only, on the stimulus but have always emphasized concomitant effects of central factors, as, for instance, in the case of visual size.

For the notion of time I retained Brentano's original conception, that it depends on continued existence, with a subjective backward relegation of all mental contents, during a short period of time. These "continued" contents, however, seem to me non-perceptual; which is of especial importance in the much-discussed question of the comparison of successive data.

The question as to the difference between mere conception and sensation, finally, is another problem of phenomenology. Purely sensory ideations—this was the result of my thorough investigation—are phenomena of the second order, which differ from those of the first order mainly by their very inferior vividness and fullness, as well as by some other characteristics.

In so far as they rest upon associative causes, the laws of their origin (reproduction) may be brought under the formula of "contiguity" or "complementation," besides which no special law of similarity is necessary. It may be questioned, however, whether reproduction ever takes place in a purely mechanical manner, or whether there are always certain functional activities involved. Moreover, there is a purely physiological type of reproduction without associative causes, which is not surprising, considering the fundamental indifference of sensation and ideation. In dreams this type is probably predominant.

b) *Psychology in the narrower sense.* The elementary psychic functions or states are characterized by definite fundamental properties: (*a*) by the peculiar relation between action and content whereby the content may consist of sensory phenomena, but also of non-perceptual elements or even of functions); (*b*) by the lack of spatial properties in self-observations (although they doubtless occur in objective space); (*c*) by specific laws of structure. Among themselves they possess many qualitative differences, and it is quite hopeless to try to trace them back to one fundamental function, as sensualism and voluntarism aim to do. In the first place, the intellectual and emotional functions are distinct, and within each of these divisions there is a hierarchy of functions such that each member subsumes the preceding: in the intellectual sphere we have perceiving (distinguishing), combining, conceiving, judging; in the emotional realm, the passive and active emotions. These as a whole are based in turn on certain intellectual functions, to which, however, they are added as new non-deducible material. All these relations present a picture of various structure, whose peculiarities have not yet been fully described. Not the least of Brentano's merits lies in the fact that he realized the importance of this task and accomplished a large part of it. Among his pupils, Marty, Meinong, and Husserl especially have worked along the same line. Lotze, before Brentano, called attention to the peculiar structure of the functions of consciousness, especially that of "relational thinking." After Brentano, though probably not at his suggestion, Dilthey emphatically advocated a structural psychology. His interests and achievements, however, were distinguished rather by a delicate and sympathetic understanding of psychic connections in general, of the spiritual history of individuals or of groups, than by close analysis of elementary psychic structures—"microscopic psychology," as Brentano used to call it.

My treatise concerning the concept of emotion was directed mainly against the sensualistic definition of James and Lange, while, in my work on sensory perception I treat the sensuous feelings as genuine sensory phenomena. The later thesis I had to defend against misunderstandings. It is not really as revolutionary as it seemed to some people; quite aside from the fact it was simply a restatement of an older theory, which has long been known to psychologists, especially in England, I did not deny the close instinctive connection of this class of sensations with acts of pleasure and displeasure, of desire and disgust, but had emphasized it everywhere and for this very reason had chosen the expression "feeling-sensations." The only exaggeration was the incidental statement that expressions such as "pain" or "pleasure" (referring to physical causes) denote mere sense data. In everyday life their meaning generally includes those instinctive emotions.

Through the entire mental life of man we perceive a dividing line which separates, in every domain, the higher from the lower functions; this dividing line is posited with the occurrence of general concepts. No matter how many attempts have been made to identify these with individual conceptions, the results cannot bear critical examination. Of course, to describe their effectiveness in shortening the process of thinking, etc., is not the same as to analyze their character—just as the physiology and anatomy of the lung are two different matters. Among emotional functions the affective and volitional processes presuppose certain concepts, just as logical thinking does among the intellectual functions. Wishing is the desire for something which is conceived somehow as valuable and as a consequence of my momentary affective state. Both conceptions, that of value and that of causality, in their more general and most primitive form, are discovered through our inner perception of the lower cravings which are prior to volition. The will, therefore, cannot be a primitive element, but only an evolutionary product of the intellectual life.

In the animal world we seem to see pretty clearly of what mental life is capable without conceptual thinking—and it is a good deal. But no a priori prejudice would detain me from admitting the beginnings of higher functions, if the facts sustained such a theory. But in that case, too, the first traces of conceptual thinking would have to be taken as something specifically novel. Although the physical development of the "new brain" may progress continuously, its psychic counterpart cannot proceed without some discontinuity. But then nature does take a leap occasionally, probably even in the physical field (quanta, heterogenesis, mutations), certainly in the psychophysical, where even the appearance of every kind of sensory quality doubtless represents such a leap. And does not the most miraculous leap occur every time the physical process of conception and embryonic development give rise to psychical life? The discontinuities are merely hidden and toned down, as it were, by the fact that the new phenomena appear at first in such tiny beginnings; but qualitatively there is a new thread in the tissue. This does not affect the inherently determinate evolution of the world.

Among the fundamental problems of general psychology, the question of unconscious mind is still one of the most urgent. Unconscious functions, strictly speaking, have not been proved by any arguments so far produced. On the other hand, there certainly are unconscious predispositions, such as all psychic activities leave behind. Besides, I consider unconscious or, better, unnoticed partial contents of the phenomena possible and real. They form the lower boundary line of the various degrees of being noticed; often the slightest intensification of attention will suffice to notice them. When we separate functions and phenomena, there is no fundamental difficulty in this theory.

If we admit unnoticed partial contents, we shall have no difficulty in defining the character of our perception of Gestalt, upon which certain young scientists of my acquaintance, who have done commendable work in studying its laws, would like to base, it seems, not only the whole of psychology but even logic itself.

I make a distinction between psychic functions and psychic structures, which latter constitute the specific contents of the former. Thus, from summarizing I distinguish the notion; from judging, the state of affairs; from conceptional thinking, the conceptual content; and from feeling and desiring, the passive and active value. These elements have, of course, no independent reality, like Platonic ideas, but still I should not call them fictions as does O. Kraus, who takes Brentano's later writings for his authority; that expression seems to me dangerous, and liable to being misunderstood, since it admits of a skeptical, subjectivistic, or relativistic interpretation. Structures form the starting-point and subject-matter of the science which I call eidology.

By soul I understand a unity of psychic functions and dispositions, and agree with Lotze in deeming it unnecessary to seek behind this unity a mediating or supporting "something." Since a strong will draws everything into its domain, and since those functions and dispositions which are connected with the will, and especially with the moral will, play the leading rôle in the life of the adult, the will is justly considered the nucleus of personality; and this seems to me to be the element of voluntarism. The will is not the root of evolution, but its crown.

If I wanted to draw a distinction between soul and mind, I should use the latter term for the totality of the higher life of the soul. Throughout the animal kingdom, we find embryonic stages of those social elements in language, art, community life, etc., which are based on the cooperation of individuals, and are the subject-matter of the special mental sciences; but here, again, the transition is not continuous and the novelty, in the last analysis, is always a product of conceptual thinking. In *Anfänge der Musik* I have tried to establish this fact more concretely within that art. The possibility of sympathetic "re-experience" (*Nacherleben*), upon which the entire structure of "insight-psychology" is based, arises only upon the level of strictly human developments. Any expert in intellectual history will insist on the reality of certain laws, though these may not be given in the precise form of natural laws, and I should even admit Hegel's triadic rhythm to have some plausibility in this connection.

5) *Ethics.* My views and ideas on this subject I developed almost entirely in lectures, but in my address on ethical skepticism I suggested the main points. Like Brentano, I see an analogy between the way our notions of intrinsic goodness or value are based on the apodeixis of feeling and the way our theoretical understanding rests on the apodeixis of self-evident propositions. The empirical derivation of altruism from egotism is entirely wrong. Our theory differs from hedonism, even altruistic hedonism, in that we recognize certain primary values beside pleasure, and from Kant's ethics in that we repudiate purely formal conditions. Truth, positive emotions (especially aesthetic), and kindheartedness (dispositions directed towards true values) are intrinsic values. One could find a comprehensive learned formula for this, but only at the expense of definiteness, and therefore it is impractical. A series of derived, but still very general, values, such as power, liberty, honor, etc., complete the "table of goods" (*Gütertafel*), which is not so very different from the Platonic table. Only such an ethics of goods or values can be developed logically in detail and also do justice to the actual changes of ethical evaluation by changing the coefficients, as it were, by which the abstract (absolute) values must be multiplied under different circumstances and conditions of life, in order to obtain the concrete (relative) values. This is essentially what must take place in every case of individual moral or ethical decision. The modification of abstract values for any concrete case depends upon certain perspectives, and these are assumed in our ethical reflection. The highest good or happiness (eudaemonism of the ancients) is *in abstracto* the totality of intrinsic values, *in concreto,* the totality of genuine goods which are possible under the given conditions of life for the individual and, furthermore, for humanity in general, including extrinsic goods as well. The conception of the transcendental ideal (the

Platonic idea of the good) can, of course, be derived only from empirically given true values by a process of augmentation. The question of egotism-altruism is solved thus: everything truly good is worthy of attainment in and for itself, under any condition, so that in each individual case not the point of view of ego or alter but only the greatest possible intensive and extensive realization should be decisive. Ethical action is purely objective action, as scientific cognition is purely objective judgment.

With regard to the free-will problem, it seems to me that the interests of ethics, for which alone it has any significance, are quite compatible with a determinism which considers ethical insight itself as a power to be developed by training and self-control. Freedom becomes synonymous with the possession of ethical insight, and is, therefore, not given once and for all, but evolves and grows with the whole ethical personality. Legal punishment recognizes free will only in this sense.

6) *Metaphysics.* Metaphysics can be fruitfully developed only from the ground up, as a continuation of sciences whose data it undertakes to generalize still further. Apart from the problems it inherits from the lower sciences, it is chiefly concerned with the relation of the physical to the psychical and the ultimate questions of God and immortality which everyone who would be called a philosopher must answer after his fashion, and from the life-long consideration of which even a dogmatic critical philosophy should not detain him.

Against the parallelistic point of view concerning body and soul, which was so popular with psychologists and physiologists in the last third of the past century, and was presented especially by Fechner in a most brilliant and fascinating manner, I have taken sides with the older theory of interaction, which merely requires sounder rational development; and recently this has gained ground again, even among the pupils of Wundt and Erdmann. The objections derived from the law of energy are easily answered, while the experiments of Rubner and Atwater can be fitted just as well into the theory of interaction. Parallelism is conceptionally obscure, and in view of the difference in structure of the physical and psychical it cannot be developed logically and compels us to assume a causal series extending forward and backward, for which there is not the slightest empirical evidence. It logically ends in panpsychism, which I can look upon only as a scientific fancy, and even so of doubtful charm. For nature is imbued with poetry only if we animate it with human spirit. However, the two opposing factions have converged in large measure, which is due partly to the greater refinement of the conception of substance and causality, partly under the stress of the facts; I dare even hope for a union of the two, in the not too distant future, toward a "monism of interaction and evolution." The metaphysician may also consider Spinoza's idea that besides the two attributes known to us there are innumerable other expressions of the world foundation, either in existence or in process of evolution. But here, of course, difficulties will arise out of the very idea.

The similarity of the qualities of the final particles of matter and the interaction between all contiguous parts of the world, or of interpenetrating parts (such as nerve-centers and psychic elements), cannot be accepted as ultimate facts of the world, if the above-mentioned maxims of research, e.g., the law of probability, are to hold good. A homogeneous world principle must supply the foundations of all things, and from the very first one is inclined to identify it with the spiritual ordering principle postulated for the organic world. The conflict between theism and pantheism loses its edge when we ask what the real meaning of causality, substantiality, and personality is, and what they may still signify here. What remains is the eternal dependence of every individual upon a fundamental essence, but as to the manner of being conditioned and as to the fundamental essence nothing more can be found out. Even the conception of spirituality we can understand only in a "transcendent" sense.

Likewise, the most difficult of all questions, that concerning the origin and reason of evil, remains insoluble. Whether, like the theists, we fall back upon the "inscrutable decree of God" as our last defense, or, more pantheistically, harp upon the connection of the divine Spirit with the laws of nature, or conceive of evil, even of wickedness, as a part of God's nature, and look upon the development of the world as the immanent becoming of the absolute: it amounts to just about the same thing. To many people the notion that God suffers with us and in us might even appear as the greatest comfort. Undoubtedly the struggle with the problem of theodicy has led many to pantheism, especially in its intensely mystical forms. But in these matters, just as in ethics, learned formulae serve no real purpose save that of hiding our ignorance. Even the pantheist may in the darkest hours of trouble place his life and fate in the hands of

God, and in time of greatest happiness thank his Creator that this world, full of sorrow though it be, is also full of joy, and that he was given a heart to appreciate it. It is, after all, a matter of degrees of anthropomorphism, and can be talked about only in metaphor. If even natural science apprehends the laws of the external world by means of symbols, why should we completely repudiate symbolism? Symbols are not mere fictions by any means. Only their status must be remembered lest the name of God be misused and the anthropomorphism carried altogether too far.

The consciousness that our life was planned for Eternity has never left me. Although the spiritual originates in the material and during our existence here must constantly be stimulated and nourished by sensory impressions, still it does not seem to be entirely dependent on them. A continued existence of the higher mental life, proportionate to the degree to which its nucleus, the moral personality, has developed, is thinkable, howbeit the form of this existence remains entirely unimaginable. Surely it was not narrow egotistic motives that inspired men like Lessing, Kant, and Goethe, as well as Lotze, Fechner, and Brentano, to hold to such ideas, but the feeling of respect and awe of the Eternal within us, and the senselessness of a world in which the only creation of real value arises merely to be obscured again, and finally to vanish entirely.

I need hardly mention that spiritualistic and occult tendencies never interested me. It is a matter of taste whether one likes to be taken in and whether the guitar-strumming of the mediums, their wise sayings about the future life, and their other emanations seem to be sufficient compensation.

7) *Aesthetics and Science of Music.* Reflections about the effect of art, especially of music, formed the beginning of my scientific thinking. In conferences and lectures, I have often discussed aesthetic problems, but have published only one of these lectures, *Die Lust am Trauerspiel* (1887). It seemed to me that this ancient question could not be solved by finding some one principle of explanation, but rather by observing the cooperation of all mental qualities, from the mere desire for sensation to the loftiest ethical and metaphysical ideas. Another fundamental idea was that truly artistic enjoyment does not depend on being carried away instinctively, but develops gradually, concomitant with an objective, imaginative survey, wherein the totality of actions and characters presents itself to us like a pageant. Empathy is only a way-station. Even the

ethical effects, to be artistic, must be conveyed by observation of ethical dispositions in this spirit. Only within such a setting does the defeat of the hero achieve artistic effect. Finally, I called attention to the difference between the instant-effect and the after-effect, which was elucidated at considerable length.

The other arts seem to lend themselves to the same approach. It was not my good fortune to develop it systematically for the art of music, where the determination of the aesthetic object is especially difficult, and where all important questions of aesthetics converge. Three main factors I would distinguish in the musical effect, which, however, may be combined in most different relations according to the individual: the purely sensory euphony (including the sensory effect of rhythm), the delight in the construction and technical execution, and, finally, the enjoyment of the content of the composition. In this third, highly controversial, point my ideas coincide most nearly with those of Lotze.

But my real purpose was to carry these controversies, which were discussed *ad nauseam* in the accepted aesthetics of music, into a greater universe of discourse, namely, psychology of music, and to fit this in turn into a general systematic science of music. To most professionals, even at the present day, the science of music means only the history of music. And yet, for this very art, leaving aside its profoundest effects, the conditions are extraordinarily favorable for an objective, logical understanding. Physics, physiology, ethnology, general aesthetics, and philosophy could cooperate with the history of music. My efforts to encourage such cooperation have met with gratifying approval, but also with resistance. Since the time of Helmholtz and Spitta, the philosophical faculty of Berlin has recognized the need of such a connection by requiring knowledge of the systematic branches (acoustics, tone psychology, aesthetics of music) on the final examination in musical theory.

To the systematic science of music belong, besides my works on physical and psychological acoustics, especially the treatise, *Psychology of Music in England,* and the book about the beginning of music.[5] In the treatise, which is an introduction to the later works on *Tonpsychologie,* I discussed the relation of music to language, and of human speech to the utterances of animals—with reference to Spencer and Darwin—but also the exaggerated nativism of Gurney (power of sound), who practically ignored all genetic explanations and resorted entirely to the erotic feelings of animal ancestors. Here the ground had to be cleared for some explanation based upon mnemonic experiences of the individual and upon musical thinking as a product of such experiences. Gurney, incidentally a connoisseur of music, published an answer to my observations (Tertium Quid); to this I did not reply, as I did not wish to continue the methodological controversy which Lotze has aptly called "a mere whetting of knives." Later on, especially in Külpe's school (also in England and America), the effects of single intervals were tested on many subjects and their statements carefully recorded. But these experiments, it seems to me, dealt only with superficial and accidental data and thus have little significance for real musical feeling; besides, one seemed to forget that isolated intervals lose their real effect, which depends mainly on compound chords and sequences; and that, furthermore, satisfactory judgments could be passed on such separate data only by someone gifted in music as well as in psychology, who could give prolonged attention both to the total structure of our music and to his own experiences. But even he will not presume to put the deepest thoughts concerning the whole or the details into words—and that is well.

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[5] Besides this the aesthetics of music will be found in several later publications, also in a popular article in the Berlin *Volkskonzerte,* and in some reviews.

My studies of musical ethnology and comparative science of music I have already reported in Part I. All that had so far been presented in histories of music as specimens of exotic music had for the most part been collected by travelers and was based generally on unreliable first impressions of the melodies, which, to make matters worse, were often harmonized according to modern European patterns. But, after A. Ellis had accurately determined the scales of different exotic instruments and W. Fewkes used the phonograph for making records of the songs, the way was open for an exact comparative musical science, and this was most effectively developed in our small circle in Berlin, especially by von Hornbostel. We know now, without appreciating the wonderful masterpieces of our period any less or intending to advocate a return to primitive forms, that the "world language of feeling," alleged to be universally understood, has not only undergone enormous changes in the course of time, but presents equally significant simultaneous differences on the different parts of the globe. The impressive development of harmonic music has led many, even Hugo Riemann, to the foregone conclusion that all music had to originate in triads, and that a hidden harmony must lurk even in one-part music, as though neutral thirds and other deviating intervals could be only discordant approximations to an intended pure interval. Such prejudices have been set aside; only occasionally is a false friend of ancient Greek music tempted to perpetrate stylistic outrages of harmonization. We know a great variety of musical forms, among them the heterophonic type widely used in Asia for which I suggested the name of *heterophony* in accordance with a passage in Plato's *Laws,* where probably the same musical form is referred to. We know that the wonderful expressiveness of our harmonies is hampered by certain rhythmic limitations, and that not only the ancient Greeks but also many primitive races excel us in regard to rhythm. I need not point out how much general aesthetic science, too, gains by such a widening of the horizon.

Anfänge der Musik, a little book based on a public lecture, traces the origins of music back to the practice of signalling and the phenomena of tonal fusion, summarizes the general conclusions I had reached through my comparative studies, and adduces several well-authenticated examples, chiefly phonographic; and furthermore it attempts a review of the most important basic forms of music production, as these have appeared in the course of time.

Two of my articles on the science of music are purely historical, yet even these are closely connected with theory; I mean the essay on the concept of antiquity, and that on pseudo-Aristotelian problems of music. For years, these problems had fascinated me, since they represent a kind of ancient tone psychology, which is of the highest value for the deeper understanding of the music and apperception of music of classical antiquity. How much light does the single sentence, "The consonant chord has no ethos," throw on the entire ancient musical consciousness? I did, indeed, come to the conclusion that the treatise as a whole could not have been written by Aristotle, or even during his time, but belonged to the first or second century A.D. I believe to have proved this as well as such matters can be proved. Among the few philologists who had studied the treatise, Ruelle declared the matter settled thereby (tranchée), whereas Th. Reinach, whose forced interpretations of the text I had criticized, attacked my article in such an insulting manner that I could not reply. The comprehension of many parts, here just as in meteorological and other types of problems, is possible only for one versed in that line, i.e., here acoustics, tone, and music psychology. This is especially true of problems concerning the peculiarities of octaves, fusion, and antiphony. My interpretation and correction of the text in Paragraph 14, which so far had been perfectly obscure, but is perfectly comprehensible in the light of fusion-phenomena, was approved by Usener and von Jan, later also by H. Riemann. A short notice in the *Literarisches Zentralblatt* remarked that very few people would appreciate the time and effort I had spent on these articles. The author counted himself among these few, adding regretfully: "Graeca sunt, non leguntur." This experience discouraged me to such an extent that I did not continue the history of the conception of consonance through the Middle Ages up to modern times. Later, Riemann gave a sort of continuation in his *Geschichte d. Musiktheorie im 9-19 Jahre.*

These are about the outlines of my scientific views and ambitions, which I was destined to carry out only in part. The work that I did do I should like to see improved upon now, and I am well aware of my shortcomings—of all my works, only the *Tafeln zur Geschichte der Philosophie* went through "revised and enlarged" editions. Many buyers probably mistook it for a syllabus. Thus, I had no chance to correct mistakes. But I am quite certain that the observations and experiments which I carried out with the utmost care will stand and will not have to be repeated. The general ideas, enlightening and valuable as they seemed to me subjectively, must submit to the sifting, trying test of time. Whatever truth they contain will prevail by its own virtue. I have never endeavored to found a school in the strict sense; and have found it almost pleasanter, certainly more interesting, to have my students reach different conclusions than to have them merely corroborate my theorems. I derive all the more joy and gratitude from the loyalty of the young people who, in the same scientific spirit, but by their own independent plans, continue the work of research.

HOWARD C. WARREN

In the early 1870's the atmosphere of a New Jersey suburban village was distinctly mid-Victorian. Theological opinion and practices played a dominant rôle in community life. Practically everyone went to church once, usually twice, on Sunday. There were no Sunday papers; not a train ran on the Montclair branch. Tennis, croquet, and other sports were frowned upon. In our own family (a typical one) the children were forbidden all toys and games. The same puritanical attitude pervaded the community life on weekdays. An elaborate system of tabus connected with the human body and its functions was rigorously observed. Certain topics and words were absolutely outlawed from general conversation.

These two factors, ecclesiastical domination of the community life and disparagement of the body, inevitably exerted a profound influence on the intellectual growth and moral outlook of the child. It is difficult for a youngster to distinguish between moral rectitude and social expediency. To our elders the difference was probably clear. But to the child every precept stood on the same ethical plane. It was wrong to lie, to steal, to hurt one's companions, to go about unclothed, to allude to the physiological functions, to say "damn," to bet, to be a Democrat or a Catholic or a Unitarian, to doubt the truth of any scriptural text.

Being possessed of two exemplary parents I strove as a child to assimilate all these precepts in my own life. Infantile lapses weighed on me perhaps to an unusual degree. The goody-goody books of the Sunday School library suggested the unworthiness of society life, with its parties, dancing, theater-going, and the like, though my parents did not disapprove of these amusements. The puritanic ideal appealed to me from the start and determined largely my outlook.

At the same time a persistent striving for logical consistency in thought and conduct seems to have underlain my intellectual development throughout childhood and adolescence. This tendency led me gradually but surely from a naïve, childish mysticism to a world-view of the mechanistic type. To give a few typical illustrations: At the age of six or seven I was greatly impressed by the Biblical story of Joseph, his interpretation of Pharaoh's dreams, and the remarkable fulfillment of his predictions. I had a striking dream and, after pondering over it, believed I had hit upon its meaning. In my excitement, I told my mother of my great discovery. She explained that interpretation of dreams was possible only in Biblical times; that nowadays dreams had no meaning whatever. This somehow did not satisfy me. I secretly felt that she was mistaken—that dreams *must* have a hidden meaning, if one could only discover it. The book of Revelation fascinated me some years later, and I spent hours endeavoring to interpret its symbolism into the events of medieval and modern history. At the age of about twelve I tried to work out an incantation formula, an elaborate sesame by which physical phenomena might be controlled.

Our family travelled much in Europe. The Continental Sabbath was in sharp contrast to our home customs, and what I saw pained me. When I was sixteen, at a Spanish hotel one Sunday evening our dinner was enlivened by a troupe of singers and dancers. I did not enjoy the entertainment, my attitude being one of suppressed vexation. I was surprised that my elders took the matter calmly and regarded it as a pleasant experience. A year or two earlier I was enrolled in a dancing class. The frivolity of this type of activity was repugnant, and I did my best to escape the ordeal. I went through the movements reluctantly and never really learned to dance. There may have been a sex factor involved, but I am inclined to attribute my attitude to the ultra-puritanical teachings of the Sunday School literature.

The general mental attitude which these and other incidents indicate was a natural product of the social conditions prevalent during the period from my birth in 1867 to about 1885. A distinctly personal factor must be mentioned on account of its profound influence on the writer's intellectual development. At the age of eighteen months I upset a kerosene lamp and barely escaped death from the burns. The months of suffering that followed left many marks on my character. At the age of five (when connected memories begin) I was still undergoing surgical operations, each of which entailed weeks in bed; for some time after it was necessary to lie down for an hour or more daily. This stimulated habits of thinking, and I was probably much more reflective than the average child. I usually lay awake a half-hour to an hour every night before going to sleep—a practice continued till college days, when the period gradually shortened to reasonable limits.

There grew up from this the habit of daydreaming of the continued-story sort. Throughout childhood these fancies were mainly stories of adventure and leadership; at adolescence they gradually assumed a more sexual character. There was also considerable speculation upon the nature of things—of the world, of morality, of religion. At the same time I acquired the habit of recalling earlier happenings, particularly episodes of our European travel, and of observing and remembering entoptic phenomena, trains of thought, dreams, etc.—in a word, various sorts of introspective material—more than is usual in childhood. While these practices had no bearing upon my subsequent choice of profession, they undoubtedly gave an early training which was of assistance later on.

Two tendencies, due to my unusual appearance, were of importance in directing the lines of intellectual development. One was a habit of rigid emotional repression, the other a decidedly introvertive attitude. I was very sensitive as a child about my scarred face and hand. These peculiarities were far more marked than later in life. People in the streets or the cars were constantly turning to look. I came to feel that everyone's eyes were fixed upon me. It was a tremendous ordeal to sit facing a group of people. I could not bear to turn around in our pew at church. Gradually I learned to suppress any emotional manifestations in these situations and to conceal my trepidation. Eventually this practice of emotional control became an ingrained trait.

A word lest all this be misinterpreted. The attitude of others was never unkind; only once do I recall ridicule, and that from a group of children. Very rarely, either, were there impertinent inquiries from strangers. The turning to look was quite automatic. It was the *invariability* of the act that led to the shrinking and introversion.[1]

Further, I do not believe my attitude indicates an inferiority complex in the Freudian sense; certainly it was not pathological. My atypical appearance was obvious to myself. This knowledge, though distressing, led to no chronic neurasthenic symptoms; on the contrary, the shock effects of the accident and operations gradually wore off.

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[1] Incidentally the situation furnishes a striking example of the way in which historic myths arise. In my junior year in college I chanced to tell my roommate the story of my accident. He said it was commonly believed that I had received my scars in attempting to rescue a younger sister from a burning building. Quite recently I happened to mention this to a colleague and was told that the same tale was prevalent among my psychological associates. We sometimes wonder at the numerous legends about Lincoln that grew up soon after his death. Here is an absolutely baseless tradition that spread among the close companions of a *living* man—which a single question might have disproved!

From the age of fifteen on I was probably quite normal and typical in respect to my nervous functions. Before that time an occasional dyspeptic spell brought out self-analysis and worry, a shudder at the notion of eternity in time or unfathomable space, a sense of moral turpitude and religious dread. But in general it is the story of a happy childhood, a pleasant home life, and congenial playmates.

My secondary education was received at a succession of small private schools, as it was feared the strain of competition might retard complete nervous restoration. There were never more than three or four in the class. The other children in the school were generally younger. Study was without severe compulsion and with no set examinations. An effective liason with other children of my age took place through my cousin Fred Torrey, who attended the public school, and through my younger brother Ralph and numerous neighborhood boys; there were also associations through the Sunday School. Our homestead had large grounds and was the usual playground for our friends.[2]

In 1877-78 we wintered in southern France, where Ralph and I learned French under a governess. Our studies were carried on with French textbooks. Some years later I lived for several months in a German family at Munich, where my family left me for a visit to Italy. I never gained the same mastery of German as of French; one can assimilate a new language far better at ten than at sixteen. My final preparation for college (before and after the Munich episode) was with a private tutor, Charles M. Davis, whose all-round knowledge was most stimulating. He encouraged my liking for mathematics especially; I ate up the originals in Todhunter's *Euclid* with avidity, and was inclined toward a civil engineering career when I entered college.

Acquaintance with the notion of organic evolution came a year or so before entering college. The Darwinian theory attracted me from the start. The notion of continuity or orderliness in the universe appealed to me. My attitude throughout childhood had been in that direction. It was in fact far easier to accept Darwinian evolution than the conservation of energy. For I was imbued with the idea that *doing work* meant *expenditure of energy,* that is, a loss of force, of power, of something.

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[2] My father's business was in New York; like most of our neighbors he commuted to the city, driving furiously every morning to the station just in time to catch the 8:22 (or was it 8:13?) and returning at dinner time. He boasted that in twenty-five years he had missed his train only twice.

There followed an attempt to combine the conception of the universality of natural law with a modified type of mysticism. Believing thoroughly in Biblical inspiration, I strove for some natural explanation of the miracles as actual events. Biblical prophecies were seen as cryptic forecasts of history. Conversion was a direct imparting of divine spirituality to the human individual. Henry Drummond's *Natural Law in the Spiritual World* had just come out, and its basic idea appealed to me tremendously, though the contents were disappointing. At the age of fifteen I joined the Congregational church, after a period of hesitation due to my failure to experience any sweeping spiritual transformation such as I had looked for. At the preparatory meeting I feared I might have to admit my partiality for the book of Revelation. The usual question happened to be omitted in my case.

In 1885 I entered Princeton. The freshman and sophomore courses were rather elementary: Latin, Greek, mathematics, history, a smattering of science. But an intimate friend and classmate, Harry Drummond, introduced me to Spencer, Huxley, Clifford, Tyndall, and other contributors to the great thought-wave of the time. I read these authors with avidity. The notion of the uniformity of nature was confirmed in my mind, and my deep-rooted belief in indeterminate volition disappeared after many an argument with Harry Drummond.

In freshman year the missionary movement struck Princeton. Of our college class about a dozen pledged themselves. The emotional appeal carried several men off their feet; personally I was swayed by the logic of the vocation, which harmonized with my general theory of the world and of life. The arguments advanced by Forman and Wilder appealed to me, and for a time my decision hung in the balance. In the end my parents decided me. They reasoned that I was not yet ready to choose my life-work, and that when the time came I could weigh this career as against other suitable occupations.

The problem of reconciling naturalism with mysticism reached a climax in junior year. Hitherto I had firmly believed in a distinct spiritual substance. I had accepted without question the reports of the outpouring of the divine spirit at religious meetings. No such experience had come to me, but I had had abundant hearsay evidence of its existence. I finally resolved to seek direct evidence. In the summer of 1887 I attended the Missionary Conference at Northfield, held under the direction of Dwight L. Moody. It was a delightful experience—a close comradeship of young men working under an inspiring leader. But for my special purpose it was disappointing. The experience I looked for was not present—either in myself or in others. I realized that "spirituality" was nothing more than earnest endeavor plus emotion—that inspiration was a figurative term for zeal.

Several months were needed to readjust my conception of the world to this crucial discovery—to cast out the mystical component which had been closely interwoven with naturalism and universal orderliness in my theory of the universe. When the adjustment was complete my first thought was to sever church connections; but my pastor, a broad-minded man, dissuaded me. Traditional dogmas he held to be of secondary importance; the essence of Christianity was right living. I have never regretted following his suggestion. As years passed, the notion of religion as an evolution in human society became plainer. The Hebrew folklore and the Patristic mythology have a historic value; so long as I am not compelled to accept them as literal facts I am content to remain in association with the powerful ethical movement of which they form part.

During the first three years of college my interest in mathematics predominated. I was a candidate for the sophomore math. prize, which was won by my roommate, John Brooks. In my junior year Brackett's course in physics attracted me. I had some thought of trying for the mathematical fellowship, but Brooks was obviously superior in this branch and I realized that it was useless to compete against him. Meantime my philosophical reading and thinking continued, and the courses in logic and psychology proved much to my taste. James McCosh was President of Princeton. I found him an inspiring teacher, though the trend of my own thinking was more in line with Spencer in philosophy and the British associationists in psychology. I enjoyed thoroughly a course in physiological psychology, given by W. B. Scott and H. F. Osborn; Ladd's monumental work, which had lately appeared, was used as a text. James's *Principles* was published the following year, but some of the chapters were appearing as magazine articles, which I read with great interest. I finally became a candidate for the Mental Science Fellowship, which was awarded me at the end of my senior year.

I had at that time some aspirations as a writer. In my junior year, noting my difficulty in writing fluently, I began the practice of writing a short composition every night. Taking a theme at random, I would write rapidly for ten or fifteen minutes. The subject might be some episode of the day, some problem of philosophy or conduct, some bit of fiction. Anything went, so long as I stuck to my rule and wrote rapidly. At the same time I wrote several stories and poems for the college literary magazine. The poems were rejected, but the stories won me the literary editorship of the magazine. The nightly *fliegende Blätter* proved a valuable training.

In various ways these undergraduate years were a turning point; first, in health. My tramping trips, begun two years before, had removed once and for all the tendency to dyspepsia. All traces of nervous shock disappeared. I have never since had a real illness or even an indisposition worth noting.

In college I associated with several rather distinct sets of classmates; a group of seven for meals, a quartette for whist and common study, a literary group, and scattering individuals outside these groups whom I found congenial on one score or another. I stood seventh in a class of about one hundred, the other members of our whist set occupying the three positions just above me. In this academic environment I lost the introversion and shrinking of childhood. I was no longer marked out for inspection by strangers, and quite forgot my exceptional appearance.

I gained also in power of concentrated thinking. Before this I had been troubled by irrelevant flights of thought; when studying or reading, an odd phrase or word would often suggest an idea leading to another and another, till I awoke to find myself miles away from the subject in hand. I have never entirely overcome this tendency, but it is no longer so marked or so troublesome.[3]

My worst trouble was stage-fright. Speaking before an audience always started an internal commotion, notably a violent palpitation of the heart, which lasted several minutes after I sat down. There was no real fear, but tremendous physiological excitement. I believe now that this was somehow related to my earlier dread of being gazed at by strangers. The phenomenon never occurred during recitations nor in addressing small groups, but in a large assembly it was almost overpowering. It was not till perhaps my fortieth year that the symptoms disappeared entirely. Even now I cannot bring myself to deliver an address without notes, for fear of becoming flustered and losing the train of thought.

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[3] An irresistible propensity to play upon words may be noted here. It goes back at least to a pun on the word Pekin in my earliest school years and has persisted through life. I have never regarded this as a sign of mental weakness. A logical mind should appreciate and enjoy the incongruity of antilogical associations better than a careless thinker.

In the autumn of 1889 I returned to Princeton for the fellowship work. I planned to make use of my two strongest points (logic and math.) by specializing in symbolic logic. At midyears I was appointed Instructor in the Philosophy Department and assisted in the elementary logic and psychology, offering an advanced course in symbolic logic. The work brought me into intimate contact with Dr. McCosh (no longer president, but still teaching) and Professor A. T. Ormond. Helpful though they were in many ways, I still could not sympathize with the Scottish philosophy which they taught. Dr. McCosh's psychology struck me as too poetical and too fragmentary. My sympathies leaned more and more toward associationism. My real masters were not those under whom I studied, but men whom I never met.

After two years of graduate work and teaching at Princeton I decided to go abroad for study. Germany seemed to offer the best facilities. Professor Ormond intimated that the Germans were not doing much research in logic, but were making great advances in psychology. He and others advised my going to Leipzig, to work under Wundt—of whom I had barely heard. Through my mathematical *maestro,* Professor Fine, I obtained a letter to Wundt from his friend, Professor Cattell, lately a favorite pupil of Wundt's.

In the summer of 1891 I sailed for Germany. A month at Erlangen in a German family helped me to brush up the language. The rest of the summer in Berlin; and then to Leipzig with the opening of the winter session. There was a notable group that year in Wundt's laboratory. Külpe was first assistant, Kirschmann second assistant. Meumann was working in the laboratory—he became assistant a year of two later. Titchener was completing his doctorate work, and Witmer was there for a year. Kiesow I saw much of. There were at least a dozen in the group who later became heads of laboratories in Germany and elsewhere. Frank Angell and Pace had worked there the year before, and were often referred to. As a rather immature psychologist I never got very close to Wundt.[4] But the training in his laboratory methods was invaluable. His psychology impressed me as real science, and I was thoroughly won over from speculative philosophy and mathematical logic, much as Christine Ladd-Franklin had been a few years earlier.[5]

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[4] Some personal impressions of Wundt are given in a symposium in his honor published in the *Psychological Review,* 1921, **28**, 166-169.

I formed no close friendship with any of the laboratory crowd. The intimacy with Titchener grew up later, when both of us were settled in America. I remember well the day when Titchener received the call to Cornell; we were in Wundt's lecture hall during the "academic quarter-hour" waiting for the lecturer, and T. asked me whether Cornell really ranked as a first-class university. I was not admitted to Wundt's seminar and was undoubtedly regarded by the maturer members of the group as still a mere novice in the field. My associations were mostly with American students in other departments, whom I met at the *pension,* or in other ways, and found congenial.

Two events of a personal nature hampered my work that year. One was an unreciprocated love affair, which created considerable emotional unrest. The other, a serious accident to my brother Ralph in a football game. His disappearance for several days in December was reported in the Paris *Herald,* which I read before receiving word directly. The family brought him over in February and I spent the Easter recess with them in Italy, returning to Leipzig somewhat late in the second semester. Whether this apparent lack of serious attention to work was noted by Wundt I do not know. At all events, he did not regard me as sufficiently advanced to undertake a research problem, and readily approved my decision to go elsewhere next fall.

During the summer of 1892 the Second International Congress took place in London. Many of the Leipzig men attended, of whom I now recall only two: Titchener on account of his being rather subdued and nervous; Gruber, the Rumanian, because he knew scarcely a word of English, and I undertook to pilot him around. I saved Gruber from appearing at an afternoon tea in evening clothes, and after the meetings were over, I saw him off on his train, receiving at the last moment, to my consternation, a fraternal kiss on the cheek.

The Congress was my first introduction to the larger psychological world. I recall Galton, Sidgwick, and Sully especially. Mrs. Ladd-Franklin was there, and Mark Baldwin, who had taught me French at Princeton. He was now professor at Toronto and was acquainted with my older brother Harry, who lived there. One remembers the trifling incidents of such an occasion rather than the papers or discussion. Sully, the Secretary of the Congress, had a way of taking off and putting on his eyeglasses continually. Baldwin started to give his paper and was overcome with faintness; he managed to finish, seated. I was struck with the way the discussion switched continually from English to French and German without any apparent break of thought, even when technical terms were bandied about in different tongues.

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[5] As a graduate student I had read Christine Ladd's mathematical papers. Our mental development followed the same course.

In October I matriculated at Berlin, working principally under Ebbinghaus. My personal difficulties had cleared up to some degree, and the semester was one of general progress. I was admitted to Ebbinghaus's seminar, which was focused upon experimental problems. I recall especially Ebbinghaus's energetic manner of conducting the seminar, his enthusiastic attitude toward the problems that arose, and his friendly encouragement of discussion.

I had not yet selected a subject for my *Arbeit;* a number of experimental problems were under consideration, and I was working at two rather fundamental theoretical problems, which have engaged my attention more or less ever since. One of these was the mind-body relation. I cannot recall just when the double-aspect view first appealed to me or from what source I drew it. Not from Fechner, I am sure. I have always associated it with Clifford, yet he does not seem to have worked it out in his *Essays.*

The other problem was that of mental qualities. How can these be accounted for in an atomistic universe? The term "mental chemistry" suggested by Mill and Wundt is analogical and explains nothing. I could attribute difference of quality among experiences only to a specific operation of mind or consciousness. Later on I worked out a parallel between the properties of nerve impulses and the types of mental operations. On the experimental side I made a study of mirror-writing learning, which remained unpublished.

It was about this time that I read Münsterberg's *Willenshandlung* and was greatly impressed with his psychophysical conception of voluntary activity. The controversy between innervation feelings and kinaesthetic sensations was just closing; the final articles in German magazines won me over completely to the nervous-arc concept, as it was later called.

This year Baldwin was editorially connected in some way with the *American Journal* and was responsible for reviews of magazine articles. He asked me to take over the current German periodicals; this was my first experience in breaking into print with technical contributions.

At the close of the semester Livingston Farrand and I journeyed together to Paris, stopping over at Nancy to visit Bernheim's clinic. Hypnotism and its ilk constituted the "new psychology" at that time. Bernheim allowed us to accompany him in his rounds of the hospital on two successive mornings. We were impressed with his method of treatment. In Paris I lived in the Latin quarter with a Japanese friend, Nakagawa, and attended lectures by Ribot at the Sorbonne,

It had been my intention to spend the summer semester at Freiburg under Münsterberg, whose work and standpoint appealed to me particularly. This plan had necessarily been abandoned when Münsterberg was called to Harvard, and I chose Munich, where Stumpf was located.[6] The work there was less satisfactory than the previous semesters. Stumpf had no seminar, and no laboratory, properly speaking. He had a room for apparatus, which consisted principally of auditory instruments. I was given free access to these and worked diligently with his tuning forks, studying overtones, difference tones, and other elementary phenomena. The lectures were stimulating, but were interrupted by the many church holidays which dotted the Munich calendar.

At this juncture Mark Baldwin was called to a new chair of experimental psychology at Princeton and was commissioned to inaugurate a psychological laboratory. He offered me the position of assistant, with the title of demonstrator. The opportunity of returning to my own college was too precious to be lost, even though it meant delay in attaining the doctorate. I accepted at once, and was back in time to visit the Columbian Fair at Chicago[7] before the university opened.

The laboratory at Princeton consisted originally of four rooms, a small darkroom, and a long hallway in the upper story of Nassau

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[6] It was soon after this that Stumpf went to Berlin and Ebbinghaus to Breslau.

[7] The University of Chicago buildings were in process of erection. I roomed in one of the dormitories.

Hall, a pre-revolutionary building with thick stone walls. Later on, several adjacent rooms were added, when the biologists moved to their new laboratory. In these quarters were spent the best years of my working life, from 1893 to 1924, when the time came to build a laboratory of our own.

Baldwin entered upon his task energetically. He took over the introductory course, and added a brand-new course in experimental psychology for advanced undergraduates. A graduate seminar was started, whose first fruits were his volume on *Mental Development.* I admired Baldwin from the start both as a man and a thinker. His lectures were so strikingly clear that I could not understand the charge of obscurity leveled against his printed works. He was always thoughtful of his subordinates and generous toward them. He expected good, solid work, and that I endeavored to give.

It was during this first year that the *Psychological Review* was born. Unable to agree with Stanley Hall about the policy of the *American Journal,* Baldwin and Cattell decided to start a new magazine of a more national character. The first number appeared in January, 1894. As an adjunct to the *Review* it was proposed to issue an annual bibliography. Livingston Farrand (then at Columbia) and I were selected by our chiefs as joint compilers of this annual, which was named the *Psychological Index.*

This work appealed to many of my interests. The analyzing of articles broadened my acquaintance with current literature. I was especially fond of language study, and the compiling work gave me a smattering of practically all the civilized tongues. The mechanical aspects of the task interested me, and the problem of combining a useful with a logical system of classification was attractive. For four years Farrand and I cooperated; meanwhile his interests had become more anthropological and he withdrew. I continued as chief compiler for many years.

During the Christmas holidays of 1893 I attended the meeting of the American Psychological Association in New York and read a paper. The Association had been organized the year before with Hall as president. This year Ladd presided. The connection between psychology and philosophy was still very close. Many philosophers had joined the A.P.A. in the absence of an association of their own. A considerable portion of the papers offered were distinctly philosophical in character, but there were many experimental contributions. My paper described an experiment on memory begun by Baldwin, which I had continued. In the eagerness of my exposition I forgot the time limit and was brought to a sudden halt by Ladd's gavel. Many of the older members overstepped their limits and none were called down. Far from feeling aggrieved, I have always been grateful to Ladd for driving home a valuable lesson at this early stage.

Through Baldwin I met several of the older psychologists at this meeting; James, Münsterberg, and Scripture I recall in particular. Titchener I believe was there, and many other neophytes.

Apart from the events just noted, the first three years afforded no special incident worthy of mention. It was a period of hard work—reading, teaching, laboratory research, reviewing, compilation of titles. I was busy with a study of belief for my doctorate —a step which was destined to be postponed for many a year. I carried out experiments on sensations of rotation and on the perception and counting of numbers. My research work was hampered by mechanical inexpertness and the lack of graduate observers.

From childhood on my dreams had been many and vivid.[8] I decided about this time to keep a record of my dreams, noting especially the senses involved in each. After working at this for a month or so, during which I awoke two or three times every night to make my records, it became evident that I was working nearly twenty-four hours a day; the research was reluctantly abandoned for reasons of health.

The experience in assisting and later conducting the introductory laboratory course was invaluable. Baldwin's genetic conception of psychology, threshed out in the seminar and in personal discussion, led me somewhat away from the Spencer-Bain-Sully associationism which had attracted me earlier.

During these years I roomed in a private house, with meals at a club of some eight young instructors. My local associations were mainly with this group. But more intimate were the associations built up in a circle outside of Princeton. Two former college friends (Will Dix and Fred Drummond) and I organized a small literary and musical group, which met monthly in Orange during the winter; at every meeting each member gave an original contribution—a story, poem, essay, or musical composition, which was subjected to criticism by some other member selected in advance. These meetings, and summer outings by much the same group at Squan on the Jersey coast, continued for nearly ten years, when marriage, death, and migration broke up the circle.

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[8] Even today I can recall quite a number that occurred in my earlier years. In one dream at the age of seven I realized I was dreaming and explained to my (dream) nurse, "You are not really Bibby. You are only part of my dream," which statement the dream nurse treated in the same tolerant way that the real nurse was accustomed to treat my childish chattering.

In February, 1896, I was promoted to an assistant professorship. When the matter was broached it was intimated that the authorities hesitated on account of my "modernistic" views. There were no theological requirements for most departments, but it was felt that a professor in the Department of Philosophy should be fairly in sympathy with the prevalent faith. I had been attending the Ethical Society meetings in New York and was again debating cutting loose from evangelical connections. But now the opposite motive asserted itself; how far could I go toward accepting the traditional tenets of the church? The inner debate was strenuous. In the end I was able to satisfy Dr. Patton's modest demands without stultifying myself in my own eyes. I have sometimes wondered how far the "will to believe" cooperated with the "Freudian censor" to attain this result. Historically I was the last of the Princeton Faculty to undergo a theological test.

This same year I joined the "Monastery," a group of four men who kept house in a cooperative way. I lived with these men till 1905, and experienced a genuine home life for the first time since entering college. To the Monastery came from time to time many literary celebrities, among whom the most notable and most lovable was Mark Twain. Scientists of many brands visited us too, for we represented four different departments.

In the summer of '96 Baldwin and I attended the British Association, which met at Toronto. My brother Harry, with whom I stayed, enabled us to entertain several of the foreign scientists. After the meeting a group of us took the Saguenay trip. It was there that I met Lloyd Morgan, very imposing with his long black beard and tall stature, but incongruously crowned with a low soft hat.

Each year the A. P. A. meetings widened my circle of acquaintances. In 1894 the Association met at Princeton. It was there, I think, that Thorndike, a mere fledgling, amazed me by talking up to his elders in an off-hand fashion. At this or the previous meeting Münsterberg brought down the house. Caldwell had been entertaining us during the evening with comic songs. There followed loud calls for Münsterberg: "Gentlemen, Mr. Caldwell has been singing the funny songs; now you want to hear the man with the funny English."

At a later meeting a controversy arose between Titchener and Scripture. I happened to breakfast with Baldwin and James, who were discussing the case. James suggested that the best way to settle the matter was by a psychological duel. "Let them both react at a given signal; and the one whose reaction-time is longer shall be declared psychologically dead."[9]

My first distinct memory of Jastrow was at the Cornell meeting in 1897. A number of us were gathered one evening in Creighton's house, when Jastrow came in. His beautifully modulated voice and fine phraseology captured me at once.

The New Haven meeting of 1899 brings a picture of Max Meyer, who had recently come to America. He presented a complex theory of tonal intervals which haunted and perplexed me for years.

These were years of hard work, but with plenty of exercise I kept in excellent health. In 1900 I spent the summer cycling in England, making almost a complete circuit of the country. The tour was interrupted by a trip to Paris to attend the Fourth Psychological Congress. I had several sessions with Ebbinghaus to discuss the semi-amalgamation of the German *Bibliographie* with the *Psychological Index,* and work out a common scheme of classification. I recall meeting Seashore there for the first time; and was delighted to come in contact with Binet.

In 1903, at the very end of the summer, I was startled by a letter from Baldwin announcing his call to Johns Hopkins. We had been closely associated for ten years and his departure left me like a rudderless vessel. My life had been deliberately planned to be a useful lieutenant to a brilliant man of international reputation. I had never wished to be a leader. I earnestly hoped that some more experienced man would be brought to fill Baldwin's chair. But the authorities took counsel with the philosophical wing of the department and elected to call a man pre-eminent in philosophy, but with no special training in the newer psychology.

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[9] The quotation is correct in substance only; so of Münsterberg's.

Again, when he left three years later, I was not consulted. Another brilliant man of the same type was chosen. Nothing remained for me but to put my shoulders to the wheel and endeavor to give Princeton a respectable place in the world of psychology. One of Dr. Patton's last acts before retiring from the presidency in 1902 had been to promote several of us to full professorships. This was most fortunate, for it gave me much greater strength in the contest with philosophy that began with Baldwin's departure and extended through the next fifteen years.

The year 1903-04 marked the beginning of a wholly new epoch. Our literary and seaside group broke up rather suddenly. At about the same time new family obligations arose. Uncle Morton Warren, who had had charge of our financial interests, became ill and died shortly after. Father was old and not strong physically. Of my two brothers, one lived in Canada, the other was still in precarious health. It devolved on me to take over the bookkeeping, and later to advise and eventually to manage the family investments.[10]

In this year, too, my editorial responsibilities were increased. Some time before, I had been made Associate Editor of the *Review*. Now Cattell and Baldwin decided to sever relations. Baldwin purchased the entire ownership of the Review Publications, and I obtained a substantial interest. We started the *Psychological Bulletin,* for which I assumed editorial responsibility, Judd taking charge of the *Monographs*. The *Bulletin* involved extensive correspondence with most of the productive psychologists. I came to know many of the Western and Southern men well through their chirography long before meeting them personally; I was often amazed to find how different they looked from the mental picture I had formed.

At the St. Louis Fair in 1904 I met Stanley Hall for the first time. There was to be a psychological session of some sort. I walked over to the Building rather early. A tall, dignified, elderly man whom I did not know was waiting alone on the porch. I sat down beside him and we conversed. I tried my best to place him, for he seemed well up in technical matters. Finally a group of men strolled up, and Adolph Meyer addressed him as Dr. Hall. I saw Stanley Hall many times after that. Our last meeting was at Clark the year before his death. I had come to Worcester to interview a prospective assistant, and called at Hall's residence. He took me up to his study and we chatted for about an hour, while he smoked a cigar in his curious pipe-like holder. He had just finished his autobiography and was full of the subject. I always found him affable and friendly.

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[10] By an odd coincidence my initiation into bookkeeping took place the very day on which I first met my future wife. Several such coincidences have fostered the latent mysticism in my make-up. The same two letters of the alphabet were initials of the four women who appealed to me most deeply. I received my Ph.D. precisely on my 50th birthday. My uniform good fortune after adolescence, following a childhood of suffering and ill-health, might be regarded as a mystically ordained compensation. Such incidents, like runs of luck at cards, impress me. One can find considerable delight in the pretense of magic without in the least tying it to the world of reality.

It was in 1904 that Titchener started his spring gatherings of experimentalists. He wrote me of his plan and invited me to join the group. I felt that such a move would would be more fittingly launched through the A. P. A. and declined the invitation. The plan was favored by practically all the others whom he sounded, and the meetings proved an immediate success. The invitation was renewed next year and I accepted without hesitation, attending all or most of the subsequent meetings. The intimacy with Titchener became closer from that time on. In the years of stress that followed, due to continual conflict with my philosophical colleagues, I came to rely on Titchener's keen, objective judgment. Whenever a difficulty or problem arose I wrote or journeyed to Ithaca. His advice always proved helpful.

In the spring of 1905 I married and left the Monastery. The years that followed were busy ones, with a distressing scatter of energy: teaching and administrative duties; editorial and (for a time) business care of the Review Publications; frequent attendance on aging parents and responsibility for their financial interests.[11] There seemed no time for experimental research, and the progress of writing my Association history, begun in 1903, was slow. My general health was good, but at times the nervous strain told severely.

The struggle to free psychology at Princeton from subservience to philosophy may be traced here. So long as Baldwin was at the helm, with his prestige and vigorous activity, psychology made slow but steady progress. When he left, the introductory course was given into the hands of his successor, whose interests were chiefly in philosophy. Later, an advanced course in psychology was introduced and given to another philosopher. I was fortunately left unhampered in the laboratory; the funds and the appointment of assistants were wholly in my hands. On one occasion, when the new preceptorial system made an alarming deficit in the university budget, President Wilson suggested cutting out the laboratory assistantship altogether for the time being. In the absence of Ormond, the Department Head (who was in the hospital), the President himself called a meeting and formally proposed this step. He had notified me in advance and I was prepared with strong arguments against it. The Department supported me, and the suggestion was never renewed.[12]

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[11] Brother Harry's death in 1909 shifted this responsibility to my shoulders —an odd situation for the sole professional member of a family of business men. On the death of my parents the responsibility for settling their estates fell to my lot.

This marked the low ebb of our fortunes. Princeton tradition favored a strong staff in philosophy. Any progressive suggestion on my part was overwhelmingly voted down in the Department meetings. When I asked for a second assistant they would express sympathy, but at the same time they would point out that a fourth or fifth assistant in philosophy was needed much more. It was with difficulty that I succeeded in getting permission to introduce a new course in genetic psychology.[13] Some years later, after repeated efforts, we were able to get the name of the Department changed to include psychology. In due course I was promoted to the titular chair, formerly held by Baldwin. The practical work of psychologists during the World War gave us a better standing at Princeton, and with the approval of President Hibben and our philosophical colleagues the Department was finally divided, and psychology became a separate and independent department in 1920.

In 1910, Baldwin, who had resigned from the Hopkins and had gone to Paris to live, decided to dispose of the Review Publications. The story of the controversy between him and certain supporters and contributors does not belong here, except to note that I was put in the difficult position of having to act as intermediary. Through conviction as well as moral obligation I supported Baldwin's side and interests—fortunately without antagonizing the other parties. Baldwin quite unexpectedly offered to sell the Publications to me, and this proved a satisfactory solution all around. Watson assumed editorial charge of the *Review,* Angell retained the *Monographs,* and Arthur Pierce took over the *Bulletin.* The Review Company was incorporated in 1911 and adopted a liberal financial policy. We aimed to make the magazines organs of American psychology rather than an individual enterprise.

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[12] I always found Woodrow Wilson very approachable and cordial. He had certain office hours when any member of the Faculty could see him and talk over University matters. He always listened attentively and would allow me all the time I wanted for discussion. I never found him unduly stubborn, though he was extremely hard to convince once he had thought a question through and reached a decision.

[13] This course was obviously needed. I offered it with reluctance, for I had had no training in animal or child experimentation. My experience in the animal field was negligible; I had made only casual observations of the mentally defective at the Vineland Training School and the Trenton State Hospital.

To finish the magazine history: At Watson's suggestion, the *Journal of Experimental Psychology* was started in 1916, with Watson as editor, while I took over the *Review.* The World War reduced the psychological output to such an extent that the *Journal* was temporarily suspended and the *Review* had hard sledding. The increase in cost of paper and labor caused suspension of dividends by the Company. After the War normal conditions were gradually restored. All this time I continued in virtually complete ownership of the Company. Several times I seriously considered conveying the property to the A.P.A. Two reasons held me back. The policy of the magazines was developing satisfactorily and it was uncertain how far the Association might be inclined to interfere in questions of editorial policy and shunt the *Review* into side lines. And again, my business sense rebelled at the thought of footing the entire cost, which I had paid over to the former owners of the Publications. The first difficulty gradually disappeared as the *Review* tradition became established, and in 1925 I offered the Publications to the Association at the original cost price. The offer was accepted, and when two-thirds was paid the remainder was cancelled.

The summer of 1911 was spent abroad. The Montessori system of pre-school training had just been inaugurated at Rome, and we took this occasion to visit the principal Casa and observe its working. I was impressed by the fact that their methods were based on genuine psychological principles. For several years I watched the development of the system in America and did what I could to promote it. Unfortunately Dr. Montessori never seemed to realize the need of adapting her methods to the idiosyncrasies of the English language and to the American culture-type. The natural evolution of the system was hampered by her insistence on training her teachers along Italian lines, and in the Roman atmosphere.

From Rome to Frankfort, where I spent several days in consulting with Wolfgang Köhler on a revision of the *Index* classification. I have seldom been lucky in my predictions, but here I made two happy guesses. To my wife I imparted my firm conviction that the young man with whom I was conferring was destined to become one of the leading German psychologists. And at a gathering which Schumann arranged, on being asked who was president of Princeton, I stated that our late president was now Governor of New Jersey and was likely to be the next president of the United States. My prediction was recalled later by several of the company.

At Brussels I attended the Pedological Congress, where I renewed several old acquaintanceships and met many foreign psychologists for the first time. De Sanctis, Decroly, Joteyko, and an interesting Russian, Natalie Maltzowa, come especially to mind. My linguistic training enabled me at one session to introduce speakers in German, French, and Italian, but the Flemish was beyond me.

It was at about this period that behaviorism began to emerge. The Würzburg school in Germany and Bekhterev in Russia introduced me to the notion of objective methods. Max Meyer and others in America were advocating the investigation of human behavior as furnishing exact quantitative data. Watson had already reached his radical standpoint, and we occasionally discussed it together. I could not follow him in his rejection of introspection, but I was quickly convinced that the behavior method was an important adjunct to introspection. I recall urging Watson time and again to publish his epoch-making paper, which he feared might lead to his ostracism from psychological circles. When his article appeared it did create a furor, but the result was by no means disastrous. The younger psychologists adopted the new conception with enthusiasm and hailed Watson as a second Moses. The *Review* gave ample opportunity to the new school for publication, and Watson was chosen President of the Association in 1915 over many of his seniors.

My own attitude was eclectic. I felt the need of both the introspective and behavior methods in psychology. Later on I came to realize more clearly that, from the double-aspect standpoint, the operations of the nervous system underlie both consciousness and response; since then I have urged continually the importance of investigating the phenomena of nerve physiology as a more fundamental psychological method than either.

Editorial interests made it imperative to attend every annual meeting of the Association in order to keep in personal touch with contributors and reviewers. During its earlier years the policies of the A.P.A. were controlled by a few of the older and more prominent members. There were indications that the society might develop into a sort of close corporation. I cannot recall in what year Witmer made his vigorous protest against "ring rule" at the annual meeting, nor whether its effect was immediate. At all events, the election of president and council members was eventually placed in the hands of the society at large, and the new policy proved successful. Just before this plan went into effect I was chosen to the Council and in 1913 to the presidency—being the last one elected to that office by the Council.

The incoming president of the A.P.A. assumes duties immediately after election. During the spring I was consulted by Professor Mecklin of Lafayette, whose progressive ideas in psychology and use of Angell as a text were disapproved by the college administration. In June he was dismissed summarily. Mecklin was rated at that time rather as a philosopher than a psychologist. I accordingly brought the case to the attention of McGilvary, the President of the Philosophical Association. The Professors' Association had not yet been formed, and earlier attempts to raise the issue of academic freedom had met with no great success. McGilvary at first doubted the wisdom of our taking action; but after some correspondence we decided to appoint a joint committee of the two Associations, with A. O. Lovejoy as Chairman. After a thorough investigation the Committee reported at the Christmas meeting[14] to the effect that Mecklin had been dismissed solely for using textbooks and teaching principles which were commonly accepted by recognized authorities in psychology and ethics. It is a matter of history that the president of Lafayette resigned shortly after the report was published. That there was any connection between the two events was denied at the time.

There is no doubt that the Mecklin investigation stimulated the formation of the Professors' Association. It was the forerunner of several similar investigations in the next few years, and undoubtedly played a part in the establishment of permanent professorial tenure in several of the larger universities. I attended the conference in Baltimore in November, 1913, at which the formation of the Association of University Professors was planned, and served for a time on the committee on academic tenure. But I was more interested in having the Association promulgate a definite code of professional ethics, and was later transferred to that committee. This committee proved to be more concerned in deciding specific cases than in formulating general principles, and I eventually lost interest and resigned.

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[14] Both Associations met at New Haven.

It was in 1913 that we first visited Woods Hole, which from then on became our summer home. During this summer I was preparing the presidential address and handling the Mecklin case. The place seemed ideal for work, and gave me the opportunity of becoming acquainted with Jacques Loeb, whose mechanistic conception of biological science appealed to me particularly. I attended the course in physiology that season and the next—a course in which the rudiments (biochemistry) were extremely difficult, and which grew steadily more easy and familiar as it advanced, till the closing lectures on nerve physiology seemed almost axiomatic.

The presidential address was the first of a series of theoretical papers which have occupied me from time to time. Despite the mass of routine work[15] it was possible to plan out and write short articles, though my Association history progressed slowly. In 1916 the first chapters of this volume were ready, and it occurred to me that they might be used in another connection.

Circumstances had compelled me to discontinue work for the doctorate twenty years before. By this time the majority of my colleagues had assigned me the title of doctor in conversation and correspondence. Why not legitimize this? Why not satisfy family pride by wearing a full-sleeved gown at Commencement? Watson intimated that the Hopkins would enroll me as a candidate and might accept part of my Association book for the thesis. Accordingly, I was entered in the Hopkins register for the year 1916-17, and in view of the unusual circumstances was allowed to conduct a course instead of attending as auditor. The task of working up the minor subjects, philosophy and education, was strenuous; it was some twenty-five years since I had taken an examination, and one grows surprisingly rusty on certain points. The final oral test was more pleasant. It was a delight to be able to talk back at the philosophers and discourse on the psychological errors of Kant. With perhaps some inner reservations the examiners awarded me the degree.

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[15] At about this time the A. P. A. appointed a committee to investigate the Academic Status of Psychology. As chairman I found it necessary to consult some hundreds of catalogues and compile the results of a countrywide questionary. Later the chairmanship of the Committee on Terminology involved endless correspondence with the committee members.

It was this same spring that the United States entered the World War. The Experimentalists were in session at Harvard when the declaration was made. The morning papers of April 7 announced the momentous step. That same afternoon we held a special meeting to consider how psychology could best assist the country. Titchener, not being naturalized, absented himself from the meeting but showed the greatest interest. An army officer was present by invitation and offered suggestions. Several lines of work were decided upon and tentatively arranged, subject to confirmation by the A.P.A. The "Army Tests," and other types of research which proved useful in the War, were initiated as a result of this meeting.

A sabbatical was long overdue. Editorial and financial responsibility for the Review Publications had prevented me from taking it. The special curriculum which the War brought about now made it possible. I claimed the privilege of taking at least one year off in every twenty-five, and it was granted. The year 1917-18 was spent in New York, and was devoted to writing the text on *Human Psychology*. With abundant free time this book was practically completed within the year, and several articles written besides. This was in marked contrast to the halting progress of the *Association History,* which took nearly twenty years to complete. It suggested a plan of dividing my time between teaching and writing. In 1920 I was placed on half time; since then I have taught only during the first semester—an arrangement eminently satisfactory to me, though it has certain obvious defects from the university standpoint.

Psychology had now become a separate department at Princeton. The problem of securing an adequate laboratory building remained. Fortunately we had a good friend in the person of Henry L. Eno, who had done experimental work himself and was attached to our Department as Research Associate. A man of means, he had just received a considerable bequest after a long contest by Columbia. He now came forward and donated a large part of the funds necessary for erecting a laboratory at Princeton. The building, completed in 1924, was appropriately named in his honor. Henry Eno was a Yale man; we were fortunate to receive this windfall instead of Yale or Columbia. With the call of Langfeld as Director of the Laboratory, my responsibilities were considerably lessened. I was able to take another sabbatical in 1926, which was spent in travel and recreation—my first complete respite from work since 1893.

The Seventh International Congress was held at Oxford in 1923. As the University was not in session, we were housed in New College, eating at Commons, and returning from evening sessions at ten o'clock under penalty of being locked out for the night. My acquaintance with many of the continental psychologists, interrupted by the War, was renewed. Janet, Piéron, and Claparède were there. C. S. Myers presided. Köhler had fulfilled my prediction; he had not indeed become Kaiser, but his laboratory occupies part of the Kaiser's palace.

It was interesting to compare this meeting with the London Congress of 1892. During these thirty-one years psychology had grown from infancy to maturity. The prominent figures at the London Congress were all missing at Oxford.[16] The majority had passed away; the remainder—Lloyd Morgan, Baldwin, Ladd-Franklin—had reached the emeritus stage. The neophytes of '92 had become the "elder statesmen" in '23. The *Leitmotiven* of the two meetings were entirely different. Philosophy and hypnotism no longer dominated. Experimental research, animal study, and mental testing loomed large. Many of the topics on the Oxford program would have seemed incomprehensible or unpsychological to the earlier generation.

The period between these two Congresses virtually covers my active professional life.[17] During these years my conception of the universe and my scientific attitude advanced steadily along the lines indicated at the outset. The thoroughgoing mysticism of childhood disappeared completely, except as a matter of recreation and fancy. From the very beginning my logical sense had predisposed me toward natural law and uniformity. During adolescence I tried to apply these principles to the occult forces with which popular tradition has endowed the universe. Gradually the evidence for the existence of these mysterious agencies was seen to be baseless. At the very beginning of my professional life I had reached a thoroughly mechanistic standpoint, and this conception has satisfied me ever since. Yet I realize that the range of scientific discovery is still very fragmentary, and I have never accepted mechanism as a dogmatic creed. I regard it merely as a working hypothesis.

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[16] Professor S. E. Henschen of Stockholm and I are apparently the only two persons who attended both Congresses.

[17] The Eighth Congress, which I attended at Groningen in 1926, and the Ninth at New Haven last year, may be mentioned to complete the historical record. At present my chief literary task is the compilation of a *Psychological Dictionary.*

The traditional theology has appealed to me less and less as the years go by. The nature of the First Cause is still unsolved, and is certainly not revealed in primitive literature. Fortunately these problems lie outside the scope of present-day psychology; they have never been discussed with my classes. During the past quarter century Princeton has broadened. The University is no longer dominated by the local theological seminary; today no one is questioned as to his beliefs, provided he confines his teachings to his own field.

As regards social institutions, I have not been satisfied to accept the traditional structure of society uncritically. Personally I dislike titles and insignia of rank;[18] the Quaker attitude rather appeals, though not their practice of emphasizing dissent by quaint modes of speech and eccentricities of dress. I could never bring myself to join Phi Beta Kappa or any of the American patriotic societies, because of their intellectual or hereditary prescriptions for membership.

My psychological training early made me sympathetic to the idea of woman suffrage. Male and female mentality differ considerably, but I could not discover any natural inferiority in the latter. Though untrained for public responsibility, women's experience in household economy seems to make them in some respects better fitted for civic duties than men. This point was often brought out in my genetic lectures. Before 1920 it always caused a shuffling of feet and ironical smiles. Since then it has been treated by the class as almost self-evident.

The traditional scheme of family life seems open to challenge. I can find no objective basis for the widespread sentiment against divorce. Trial marriage would seem to offer a safeguard against acute difficulties in many cases, and might well be given an opportunity to justify itself. The stigma attached to illegitimacy of birth is certainly illogical. In most cases the odium should properly attach to the father—not to the mother, and least of all to the child.

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[18] It is perfectly feasible to write a scientific text without using the titles, Dr., Mr., Sir, Lord, etc., in citation, and without any one noting the omission.

The Anglo-Saxon tabu against the human body and its functions appears quite unnatural,[19] especially in a civilization which glorifies the nude in statuary and painting. The recent tendency to abolish many of the Victorian conventions seems to indicate a healthier attitude of mind in the community.

In social as in theological questions my viewpoint has been reached through observations and reasoning rather than personal discussion. I have never run counter to accepted standards, nor been tempted to combat public sentiment. "A decent respect to the opinions of mankind" characterizes the evolutionist, as distinguished from the revolutionary.

The development of my psychological attitude can be readily traced in my writings and need only be touched upon here.[20] My initial associational basis has been broadened to include evolutionary principles. The double-aspect theory has appealed to me for years as the best hypothesis to explain the mind-body relation; the actual working out of this relation will depend upon our ability to discover the true character and principles of neural activity. The doctrine of relativity and the newer physicochemistry have made the nature of experiential qualities less comprehensible today than they seemed a generation ago; but one cannot ignore the existence of these "qualities" as characteristics of phenomena.

Since 1910 and 1911 I have been increasingly impressed with the value of the behavior method in psychological research. Yet I cannot see the force of the criticisms leveled by the radical behaviorists against the introspective method and data. All phenomena of nature are proper subjects for scientific investigation—conscious experiences as well as atoms and responses.

Toward the configuration psychology I have taken a receptive attitude. I can see no real conflict between its standpoint and that of associationism. The notion of Gestalt does not seem essentially different from the conception of qualitative synthesis which I have held for thirty-five years.

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[19] I have personally known individuals whose sense of nudity was associated with the feet, the shoulders, the head. If only this part was covered, the feeling of shame disappeared.

[20] I have never been seriously interested in "psychical research," though, as a matter of routine, its claims should be thoroughly examined by rigid scientific methods.

This brings to a close my motion-picture show. If there are more reels to follow, they are still in the making. Were I pressed to supply a title to this exhibit, it might perhaps appropriately be called "From Mysticism to Mechanism."

THEODOR ZIEHEN*

I was born in Frankfort-on-the-Main on the twelfth of November, 1862. While I was studying at the Frankfort *Gymnasium,* which I attended from the sophomore to the senior year, I was equally interested in natural science and mental science. The two subjects were blended gradually in my mind when finally my interest concentrated on the theory of understanding. Even then I read Plato, and Kant's *Kritik der reinen Vernunft* as well as many other great philosophical works. The meager instruction we received in psychology and logic was not inspiring. Very early I became convinced that all experience is psychic, i.e., is identical with what we generally call mental processes. For a long time I stood very close to Berkeley's absolute idealism; Berkeley's works, however, I read much later. Through his influence, psychology assumed a very special significance for me. It seemed even then the starting-point in science for the formation of a comprehensive, unified conception of the world.

Material reasons compelled me to take up the study of medicine. In Würzburg, where I spent four semesters, I eagerly continued philosophical studies besides my medical course, and spent many hours wandering through the vineyards above the Main, with a much-read copy of Kant's *Kritik der reinen Vernunft.* I also studied mathematics with much interest. The lectures of Professor Prym (well-known by his work on the Theta-functions) exerted a lasting influence on me. Later on, Professor Prym frequently advised me in regard to the continuation of my studies in mathematics and theoretical physics. I attended also the stimulating lectures on the theory of understanding by the privat-docent Georg Neudecker. Psychology scarcely existed in Würzburg at that time.

From Würzburg, I went to Berlin where I spent five semesters studying medicine. Financial difficulties forced me to take my medical state's examination (by a special permit of the Minister of Education) after only nine semesters of preparation. During my course in Berlin, I specialized in psychiatry and neuropathology. The former especially appeared to me a compromise between medicine and philosophy. I also worked regularly for four semesters in the laboratory of the physiologist, Hermann Munk. A result of this work was my thesis, "Krämpfe infolge electrischen Reizung der Grosshirnrinde," and a short anatomical essay. The studies in psychiatry and brain pathology greatly influenced my psychological views in general, since they confirmed my conviction of the unequivocal connection of all psychic phenomena with physiological processes of the brain. I have scorned, however, then and later, every materialistic theory of understanding. To me the great problem was to harmonize the general parallelism between psychic phenomena and the physiology of the brain with the idealistic viewpoint, which I never gave up entirely, that "all experience is psychic."

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*Submitted in German and translated for the Clark University Press by Mrs. Thekla Hodge.

I did not attend psychological lectures in Berlin, except a course on physiological optics; but I read a great many psychological books. Wundt's *Grundzüge der physiologischen Psychologie* exerted perhaps the greatest influence—in a *positive* way by teaching me the experimental methods of psychology, the great importance of which I recognized at once; in a *negative* way by confirming my doubt of the author's theory of apperception. I was convinced of the absolute lawfulness of all phenomena and all activity, as well as of the *absolute lawful coordination of the psychic with the physiology of the brain,* so that the existence of an apperception, in the sense of Wundt, seemed to me quite improbable. I often wondered whether or not the old English psychology of association, which I knew then only superficially from the works of Locke and Hume, whose shortcomings were always quite evident to me, might not be changed so that all psychic processes (except, of course, the sensations) could be conceived as lawfully unitary without assuming an apperception or some similar function.

After passing my medical examination, I was for almost a year assistant in the private insane asylum of Dr. Kahlbaum in Görlitz (the real discoverer of the so-called dementia praecox or hebephrenia and katatonia). There was not much chance for psychological experiments, but I learned a great deal from Dr. Kahlbaum about the exact determination of clinical psychological conditions, which was later useful for my normal psychological investigations.

In May, 1886, at the suggestion of Otto Binswanger I was appointed *Oberarzt* at the Psychiatric Clinic in Jena, where I was admitted to the Faculty through my treatise "Sphygmographische Untersuchungen an Geisteskranken," in which I demonstrated also the influence of moods (Stimmungen) and emotions on the pulse curve.

As Professor Binswanger most generously placed the funds of the clinical institute at my disposal to purchase the necessary instruments, I was now able to experiment on all occasions, although in Jena I did not have the rooms and equipment that a psychological institute should have. Gradually, I equipped a small private laboratory—the apparatus I took with me wherever I went. In choosing my instruments, Professor Münsterberg (who was then still in Freiburg) often advised me. Since I had not had the opportunity to work in a psychological laboratory while studying in Berlin, it was a difficult task to find my way—by the method of "trial and error"—through the labyrinth of psychological experimental technique. Later, in Jena, I gave systematic instruction on the technique of psychological experiments. Through the friendly cooperation of the Professor of Pedagogy, H. Rein, I had the opportunity of making psychological experiments with the children in his seminary-school.

I had developed my own system of psychology, based on my observations, which I discussed in my lectures on physiological psychology. In line with these lectures was the textbook on physiological psychology, published in 1891. The essential points of this book I shall discuss later on; I will add here only that in later editions, up to the twelfth in 1924, my fundamental principles have remained essentially unchanged. Only in one essential point did I have to make changes in the second edition (1893). Moods and emotions—in general, the accentuation of feeling in sensation and conception—had not been sufficiently discussed in the first edition. Numerous observations during the years 1890-1893 had brought greater clearness; but only during long lonely walks in the Scottish Highlands in the summer of 1892 did I discover that theory of feeling which combines and unites all those observations (the theory of irradiation and reversion).

In the summer of 1900 I was called to Utrecht as Professor of Psychiatry. I was granted the means for the building of a laboratory in connection with the insane asylum, so that I could conduct many experimental psychological investigations. There was no clinical institute in Utrecht at that time. In the year 1903, I accepted a call to Halle as Professor of Psychiatry and Neurotherapeutics. After six months, I was called to Berlin to a similar position. Here also I fitted out a psychological laboratory in the *Charité*. My manifold duties here, my direction of the Clinical Institute, the clinical lectures, consultations in connection with my practice, the numerous examinations, etc., seriously interfered with my philosophical and psychological work. Therefore, I gave up my position and retired in 1912 to solitude and philosophy in Wiesbaden. Here, after an interruption of ten years, I could concentrate once more on my work in philosophy and psychology. After five years (1917), I accepted a call to Halle as Professor of Philosophy and Psychology, where I have remained up to the present time. The various mental influences which have affected me during these years I may summarize as follows: I studied, of course, all important works in the world's psychological literature, finding and absorbing much that was interesting and stimulating, adding and combining this with my own thoughts and solutions, and retesting much of the experimental work. Especially stimulating I found Helmholtz, Hering, Fechner, Spencer, Mach, and Brentano; and, among the contemporaries still living, G. E. Müller and Stumpf. Furthermore, I cannot emphasize too much that the great psychological thoughts, contained in many purely philosophical works, have influenced me very decidedly. Not the least among these was Kant, whose *Kritik der reinen Vernunft,* for instance, contains the main principle of the Mach-Ehrenfels' Gestalt psychology.[1]

The general plan of my psychology is about as follows: Introspection and objective observation must work together (determination of behavior); the former is even more important for human psychology than the latter. An essential, indeed indispensable, element in the development of scientific psychology is the experiment, although I have never maintained that it is the *only* method. In my *Grundlagen der Psychologie* (Leipzig, 1915), which is an essential complement to my textbook, I have dispensed almost entirely with the real experiment, i.e., with the *intentionally produced, systematically varied observation.*

Psychology is only scientific, when, in the first place, it introduces to any extent scientific experimental methods and when, in the second place, it is based on observation and excludes purely speculative combinations. "Psychic" and "conscious" are identical in psychology. Whoever assumes the existence of "unconscious psychic processes" must state some characteristic feature which these have in common with conscious psychic processes, but which they have not in common with purely physiological processes. Since this is impossible, the assumption of unconscious psychic processes must be condemned, without any detriment, of course, to the hypothesis of hylopsychism, as I, as well as Spinoza, Fechner, and Paulsen, interpret it, differing somewhat from its interpretation in the *Erkenntnistheorie*.[2] Although I thus consider the so-called unconscious psychic processes as purely material (physiological), still I have never denied their extraordinary importance for the generation and development of mental processes. My whole theory of latent conceptions and of constellation fully recognizes this significance, quite independently of Freud and his theory of psychoanalysis.

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[1] Cf. my treatise on "Kategorien und Differenzierungsfunktionen." *Vjsch. f. wiss. Phil. u. Soziol.,* 1915, **39**, 133, 312.

In regard to the psychology of the sensations, I consider a short article, generally overlooked, of some importance, in which I set forth the psychophysical theory that a minimal change of *intensity* of sensation corresponds to the *differential,* and a minimal change of *quality* of sensation corresponds to the *variation* (in the mathematical sense) of the process of stimulation in the cerebral cortex.[3] Not without importance seems to me is also the change I made in the Bain-Lotze theory of local signs. Lotze's view is tenable only when we admit local signs phylogenetically, in the sense which I have already explained. I may add that in my *Erkenntnistheorie* and also in my *Psychologie,* I have repeatedly pointed out the essential differences between locality and temporality of sensation. The fundamental fact of spatial localization, as I have presented it also in all editions of my textbook (12th ed., p. 116), does *not* exist for temporality, and the resulting problem and its solution has so far not been considered seriously enough, it seems to me.

In regard to the *formation of conceptions and processes of thought,* I should stress especially the theory that besides retention there are only three other irreducible intellectual functions, from the cooperation of which all processes of the formation of derived conceptions and of thought may be explained (analytic, synthetic, and comparative functions). This theory, like many others, I discussed in my lectures long before it appeared in print.[4]

In the field of the psychology of thought my theory has generally been called the psychology of association. I do not object to this name, although it refers to only one point in the entire theory—an important one, it is true. I do protest, however, when because of superficiality, or just to facilitate criticism, they attribute ridiculous statements to the psychology of association and especially to my theory of it, statements which I never made.[5] In the first place, I wish to stress the fact that my psychology of association differs in essential points from the theory of the former English associationists (Locke, Hume, Hartley). The latter maintained that the factors of contiguity and of similarity suffice,[6] so that recently when, for good reasons, the association of similarity *had* been reduced to the association of contiguity, there remained, as the only factor of association, contiguity. But, on this foundation, to refer the process of thinking to the laws of association is simply impossible, as I have always contended. One was obliged either to dispense with the idea that the process of thinking is subject to any laws or to demonstrate the presence of other factors of association subject to other laws than contiguity. I have chosen the latter way and believe to have demonstrated as additional factors the clearness, intensity, and constellation of latent conceptions. Only in this manner, especially by considering the factor of constellation, can one do justice to the enormous variability of thinking, without questioning its lawfulness.

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[2] Cf. in connection with these questions my treatise on *Die Beziehungen der Lebenserscheinungen zum Bewusstsein,* Berlin, 1921.

[3] The short remarks by Becher (*Zsch. f. Psychol.,* **48**, 447) hardly do justice to my ideas.

[4] Discussed at length for the first time in the above-mentioned treatise, *Kategorien und Differenzierungsfunktionen.*

Still more unjust is the accusation that associational psychology attempts to *explain* the processes of thought as to their content. That never occurred to me. I have always maintained, as simply self-evident, that not even the simplest quality of sensation like "blue" can be *explained* by physiological processes in the cortex. Still more impossible have I considered this for the processes of thinking. To my mind, the psychology of association is characterized only by the statement that the sequence of a series of conceptions in the disparate association of ideas as in the association of judgment, including the most superior processes of thinking and the voluntary movements connected with these, are *necessarily determined* by laws, psychophysiological laws, the so-called laws of association. The *content* of the conceptions and their combinations is not produced by the laws of association. That is why the very discovery of the significance of the, by me, so-called constellation, for which one had formerly only occasionally cited examples, whose real nature and importance had not been recognized, had become the decisive element in my whole theory of psychology. Through the factor constellation, the assumption of an apperception or any other "free" mental function, i.e., one not subject to laws, became superfluous.

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[5] Generally they quote from editions which appeared more than thirty years ago, and even these are misquoted.

[6] Hume added causality, which was evidently wrong.

If at Moment 1, an excitation complex $A\ B\ C\ d\ e\ f$ is present (capitals indicate cortex excitations accompanied by psychic process; small letters, those not accompanied by psychic processes), then, according to the laws of association, there follows necessarily at Moment 2 an excitation complex of perhaps this form: $A'\ b'\ C\ D\ e'\ f'\ g$.[7] To the excitation complex $A\ B\ C\ d\ e\ f$ of Moment 1 corresponds a mental (conscious) process, which I designate with Greek letters as $\overbrace{\alpha\beta\gamma}$, which may be for instance a composite conception. The bracket indicates that $\overbrace{\alpha\beta\gamma}$ is a *united complex* which in its completeness corresponds to the whole physiological complex $A\ B\ C\ d\ e\ f$. In a like manner there corresponds to the excitation complex $A'\ b'\ C\ D\ e'\ f'\ g$ at Moment 2 a mental process $\alpha'\gamma\delta$. I only claim that $A'\ b'\ C\ D\ e'\ f'\ g$ follows $A\ B\ C\ d\ e\ f$ according to physiological laws, and hence that $\alpha'\gamma\delta$ necessarily follows $\alpha\beta\gamma$, since both correspond to the two physiological processes according to the laws of psychophysiological parallelism. But now they impute that I claim that $\alpha\beta\gamma$ and $\alpha'\gamma\delta$ can be *explained* as to content by the laws of association. That is, of course, nonsense. For instance, that the conception "duty" ($=\alpha\beta\gamma$) corresponds to the complex $A\ B\ C\ d\ e\ f$, and the conception "categorical imperative" ($=\alpha'\gamma\delta$) to the complex $A'\ b'\ C\ D\ e'\ f'\ g$, has nothing to do with the laws of association, but corresponds to entirely different laws, which I have called parallel lawfulness and to which belong, as a sort of special case, in the field of sensations, the specific qualities of the senses. After I became aware of this absurd misrepresentation of my views I protested innumerable times against it,[8] but it was, of course, much easier to uphold the misinterpretation and thus to facilitate the refutation of the opponent. Of course, no psychologist of any note took part in these misinterpretations.

The point I just discussed in the example of the sequence of two *unconnected* composite conceptions holds also for the succession of *conceptions* in our judgments and conclusions. Only here are added the laws which regulate the connection between the processes $A\ B\ C\ d\ e\ f$ and $A'\ b'\ C\ D\ e'\ f'\ g$ so that now the judgment $\overbrace{\alpha\beta\gamma}\rightarrow\alpha'\gamma\delta$ corresponds to the successive complex $A\ B\ C\ d\ e\ f \rightarrow A'\ b'\ C\ D\ e'\ f'\ g$. The judgment is here again determined entirely according to the laws of association, but its content again results from an entirely different lawfulness. I have always pointed out these facts and with some emphasis, even as early as 1891 in the first edition of my textbook (p. 127). James's theory of "the stream of consciousness" I did not read until later, and it only confirmed my views. Likewise have I considered conceptions as isolated *things,* but always as connected *processes.* Finally I hold that for the content of a conception or conception-constellation, also the *latent* elements (in the given example $d\ e\ f$ *and* $b'\ e'\ f'\ g$) are of essential importance, and are represented in the united complex $\overbrace{\alpha\beta\gamma}$ and $\alpha'\gamma\delta$. They correspond to the "halo" of James and to the "nimbus" of a later presentation.[9]

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[7] The prime indicates that slight changes have occurred, i.e., of e to e'.

[8] Cf., for instance, only recently, *Zsch. f. Psychol.*, 1925, **49**, p. 127.

It is self-evident, of course, that neither $A\ B\ C\ d\ e\ f$ and $A'\ b'\ C\ D\ e'\ f'\ g$ nor $\overbrace{\alpha\beta\gamma}$ and $\alpha'\gamma\delta$ are to be conceived as sums, but as relation complexes fused to a unity. Likewise does α not correspond to the isolated A, β to the isolated B, etc., but the entire complex $\overbrace{\alpha\beta\gamma}$, whose *especially prominent* members are α, β, and γ, corresponds to the entire complex $A\ B\ C\ d\ e\ f$.[10]

This viewpoint also offered a solution of the problem of *imagination,* which had always fascinated me, probably on account of my interest in aesthetics. In the first editions of my textbook and even in my *Grundlagen der Psychologie* this subject had been slighted somewhat. Not until my sojourn in Wiesbaden and through my extensive psychological seminary work in Halle, did I gradually gain a clear understanding of this subject.[11] It was my special aim to prove that even in the realm of the apparently "freest" creative activity (imagination and speculation) we find only processes determined by law. At the same time I endeavored to perfect the experimental investigation of the activity of the imagination.[12]

It was not easy to transfer all these views to the theory of *attention* and, indeed, in the first place, of *sensorial* attention, i.e., that directed to sensation. Here Wundt's theory had its firmest hold. The problem of active attention, which here is mainly concerned, was then and is today as follows: Is its essential element an *increased "clearness"* of the conception complex toward which the attention is directed and does this clearness depend on the intercession of a special "free" acting mental function (apperception or something like it), or is the essential moment an elective joining of conceptions to *one* sensation or to *one* conception complex among many, according to definite laws? As early as 1891, I reached a conclusion in the sense of the second alternative and hence considered the increase in clearness only as an important biologically explicable accompanying phenomenon. The factors connected with elective attention and determined by fixed laws I discussed rather summarily in my earlier works. Only during the last ten years, as a result of continued observations, experimental as well as non-experimental, have I been able to explain them correctly. In an analogous manner, I developed the theory of intellectual attention (attention to conception complexes).

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[9] Cf. especially my *Aesthetik.*

[10] I refer by way of an example to p. 51 of my *Grundlagen der Psychologie.*

[11] The results of this study are given most completely in *Aesthetik,* Vol. II, 1925, pp. 134ff., 148ff., 307ff.

[12] *J. f. Psychol. u. Neur.,* **37**, p. 422.

I have already pointed out the development of my views on the psychology of *emotion*. Here the main difficulty always seemed to be contained in these two problems: (*a*) What relation does the emotional tone of conceptions bear to the emotional tone of sensations? (*b*) How do relatively independent moods and emotions, i.e., emotional *attitudes,* arise from the affective tones connected with our sensations and conceptions? My answer to the first question was that the affective tone of sensation not only passes into the content of the related conception, but is often transferred to the conception itself as its attribute: we recall a pleasant experience not only as such, we often enjoy it again in our recollection. To this is added what I first unsuitably called reflection, but have called now for some time reversion of the affective tones of conceptions to the affective tone of the sensation. I believe that certain pathological observations by L. Meyer first pointed out the right way. If we add to this the irradiation of the affective tone from a conception to its associatively related conceptions,[13] then the complicated united total affective tone becomes at once intelligible. Thus we find also in the realm of affective tones a "halo" or "nimbus." For the psychology of art, these processes of fusion are especially important; therefore, I have investigated them with great assiduity.[14] The second question I tried to answer by the assumption, evidently corresponding to the facts, of the irradiation of the affective tone also between successive mental processes. The somewhat mysterious assumption, maintained at the time of Wundt and often even now, of a total reaction of the *ego* or of an apperception to a sensation or conception has thus become superfluous, I believe.

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[13] "Transfert" of many English authors, e.g., B. Sully.

[14] See *Aesthetik,* p. 2, Lecture 12, and elsewhere.

In connection with these topics, I have also advanced a hypothesis concerning the so-called "emotion-producing" physiological process (Lotze), assuming that the affective tone depends on the readiness of the cortical elements to discharge.[15] I remember distinctly that this idea suddenly occurred to me during a lecture, and after a long investigation was finally published.

Special difficulties are connected with the study of *ethical* feelings. Their discussion in the first edition of my textbook was insufficient, and even in the last edition in 1924 the *single* ethical feelings have not been duly treated. Since then I have gathered a vast amount of material (observations, etc.), which I hope to work over sometime. In connection with this work I found especially useful some investigations which I had carried on for many years in the prison during my work with criminals and neglected and difficult children and young people. Gradually I greatly improved my method of investigating ethical feelings.[16] My lectures of many years on characterology are still waiting for publication.[16a]

In the psychology of the *will* I received my first incentive from Meynert and for some time I was inclined to assume that every volitional action was based on a conscious or latent *motor conception*. The treatise by Münsterberg, *Die Willenshandlung* (Freiburg, 1888), further confirmed my views. Later on I relinquished entirely this overestimation of motor conceptions, especially through my own continued investigations (also many pathological studies). Now, for instance, it seems to me extremely improbable that my voluntary act of speaking is caused by the constant intermediation of motor conceptions of speech, but I assume that the acoustic images of words can affect Broca's center directly, i.e., without the mediation of a kinaesthetic intermediator.

My conception of the will found its preliminary form in *Die*

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[15] *Zsch. f. Psychol.,* **31.**

[16] See the treatise *Über das Wesen der Beanlagung und ihre Erforschung,* (4th ed.). Langensalza, 1929.

[16a] These lectures on characterology were published in June, 1930, by Beyer u. Mann, Langensalza.

Grundlagen der Psychologie in 1915.[17] The lecture cycle on *Willenspsychologie* contains many additions (Jena, 1927).

I had never been satisfied with the one-sided explanation of acquiring suitable movements and actions by the method of trial and error. Therefore, Cole's experiments on raccoons (also his polemic against Thorndike) and Shepherd's with monkeys, but especially Köhler's with chimpanzees, interested me intensely. I saw at once that besides the method of trial and error there is another modus operandi, which is identical in the main with the so-called "practical" intelligence (designated by me as "sensory motor combination"). This theory presents many difficulties which can be removed only by further investigation. Expressions like *Ganzheitsbezogenheit* are empty words, without meaning or value. In an article for the *Zeitschrift für Psychologie,* Volume 97, I attempted an explanation of this theory from my viewpoint. Here I could without hesitation use the expression "structure of the sensory field" of Köhler and others, if that is supposed to mean the entire complex of the relations given in the sensory situation, in regard to the desired task.

I have always opposed the assumption of a "free," i.e., not determined by any law, activity of the will. It may be that the early influence of the Calvinistic faith, my parents belonging to a Calvinistic reformed community, was partly responsible for this tendency. Later on it was especially the study of Spinoza and of Kant's *Kritik der reiner Vernunft,* besides my own observations, which led me to the "determining tendency." The *Kritik der praktischen Vernunft,* with the hypothesis of an intelligible character, aroused my opposition. To this was added the influence of my intensive study, continued for years, of forensic psychiatry.

But very soon, like many other psychologists, I went still farther than determinism, as I became convinced that volitional action is not irreducible, but may be traced back to the cooperation of the other mental functions (syncretism). Even during my student years, I was strongly opposed to the so-called voluntarism, which assumes a certain specific volitional action. I am not consciously aware that I was influenced in this matter by anyone. It was rather in the main a natural, logical, almost inevitable development of my general views.

In regard to the theory of the *ego,* to which the other parts of psychology, so far discussed, are subordinate, as it were, I was at first too much influenced by Meynert, to whom Munk in Berlin had directed me. In the far too short discussion in my textbook (1st ed., p. 138), the after-effect of Meynert's teaching is very evident. Later on I fitted the theory of the *ego* as a unitary function into the structure of my psychology.[18] Criteriological speculations exerted a strong influence in this matter. The assumption of a substantial, simple, and, therefore, immortal individual Ego I have always opposed. During the War, March, 1915, I expressed my thoughts about the immortality of the soul in a half poetical dialogue.[19] On my own copy are written the words from *Prometheus* by Aeschylus: *πάντως ἐμέ γ' οὐ δανατώσει* by which I mean the non-individuality of the single human being.

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[17] Theory of the conception of time and of the *Blankovorstellungen.*

Besides this general structure of psychology, many of her special problems in various branches have interested me intensely.[20] The first impetus in this line was given probably by the clinical investigation of the insane where the shortcomings of psychiatric methods became most apparent. A treatise by C. Rieger in 1888 inspired me to develop, in the first place, a general methodology to determine the defects in intelligence. My first publication appeared in 1894.[21] In 1897, I had developed a more elaborate plan. Binet's work was still unknown to me. The progress of methodology is reflected in the five editions of my *Prinzipien und Methoden der Begabungs insbesondere der Intelligenzprüfung bei Gesunden und Kranken.*[22] Later on, as the somewhat changed title indicates, the investigation was extended to all the faculties, even to those that do not belong to intelligence, including also gifted subjects. The just-mentioned treatise on native abilities (*Anlagen*) is really a complement of *Prinzipien und Methoden.* Naturally I learned much from the numerous publications appearing each year on this subject. It was not my intention to compile all methods thus recommended. I tried rather to choose with careful discrimination among the methods I had tested and found satisfactory. I was very much opposed to Binet's general methodological standpoint, although I highly approved of certain methods recommended by him and his followers. Likewise

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[18] The most complete presentation is found in my works on theoretical understanding, and in *Der Grundlagen der Psychologie.*

[19] *Dtsche Rundschau,* March, 1915.

[20] Among these the great problem of the intelligence tests and the investigation of native ability (*Anlagen*).

[21] *Psychiatrie,* 1st ed.

[22] Last edition, Berlin, 1923.

I have always doubted the wisdom of rashly adopting norms and correlation coefficients. I have accumulated for single methods innumerable records of investigations; still I do not feel justified in setting up norms. Neither do I believe in the investigation by group tests (*Massenuntersuchungen*) and by the questionnaire method. The former is, it is true, sometimes a necessary evil in practice, but should at least never be used for scientific results.

During the last ten years I have studied with special thoroughness the analysis of native abilities (*Anlagen*) in music, drawing, and mathematics, and the correlation of development and inheritance of these gifts. The combining of these problems seemed to me especially promising. These investigations have not been finished yet. A comprehensive monograph on musical ability by V. Haecker and myself is already published (Leipzig, 1923), also an application of the results to the genealogical tree of Robert Schumann.

In connection with the psychology of the child and of the adolescent, I was especially interested in the psychology of development. Besides, there was the need of practical application in my work, already mentioned, with the deficient children.[23] In contrast to the more literary treatments of this subject, I have tried to gather facts, to analyze and arrange them, but especially to understand them.

A great deal of time and space is taken up by my aesthetic and logical investigations. The former have already been mentioned repeatedly. Although greatly influenced theoretically by Kant, Schelling, Hegel, and others (in a negative as well as a positive manner), they are based in the main on a foundation of psychology and the history of art. From Fechner I learned many things, but I differ vitally with him in certain respects, for instance, in spite of my tendency to use mathematical formulae, in the study of aesthetics, the mathematical method is of comparatively little value to me. Very different from these, however, are the investigations on logic which are contained in a larger work,[24] while the problems of aesthetics belong almost entirely to the realm of psychology. The latter is wholly separated from logic—they have, in fact, not a single law in common. Psychology teaches how the mind thinks; logic, how the mind should think. On this point I agree entirely with Husserl. Actual experience contains laws of logic which are entirely independent of human thinking (thought). Still I contend that a psychological foundation is indispensable for logic, because the very application of the laws of logic to human thought (conceptions, judgments, conclusions, etc.), in a regulative sense is what interests us. Therefore, my logic contains, besides an autochthonic, mathematical and epistemological foundation, also a very elaborate psychological foundation (pp. 316-402), which completes many important points in my psychology of thought.

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[23] The results of this work are contained in numerous essays and in the treatise, *Das Seelenleben der Jugendlichen* (3rd. ed.). Langensalza, 1927.

[24] *Lehrbuch der Logik auf positivistischer Grundlage mit Berücksichtigung der Geschichte der Logik.* Bonn, 1920.

As to the position of psychology as a science at present, much is said, especially in Germany in certain circles, about a crisis of psychology. This often much-exaggerated talk of a crisis has been refuted by Störring, Wirth, and others. The fact is that scientific psychology continues on her course of investigation quite undisturbed by such "crises." Differences of opinion will occur in all fields of research and at any time—their absence would indicate stagnation. Even in mathematics we find the struggle between intuitive and axiomatic points of view. That these differences of opinion are more pronounced today than, say, forty years ago no one will admit who experienced the struggle between the different tendencies of those times. Possibly one stresses those differences more at present, not always from entirely unselfish motives. Furthermore, I cannot admit that the differences are more profound in method or principle today than formerly, and ought to be decided by some absolute decree in favor of one side or the other. I shall illustrate this by some examples in the following lines.

Take, for instance, the contrast between objective and introspective psychology. In animal psychology we were familiar with these terms through the work of Bethe, von Üxküll, and others, since 1899. Bekhterev's reflexology belongs also to this group. My own standpoint I have already explained. I do not see, however, that this is necessarily a question of *either, or*. Why not let the objective psychologist, behaviorist, reflexologist, or whatever he may call himself, work along undisturbed side by side with the introspective psychologist, and then await results? There is no crisis at all. One might even prophesy quite safely the outcome: The objective psychologist will for many problems call upon introspection to gain his point, while the introspective psychologist will, at least occasionally, be obliged to apply purely objective observation.

Quite similar to this is the contrast, so overstressed by Dilthey, between analytical and constructive psychology. About thirty-five years ago (1894), Dilthey criticized psychology because it did not analyze and describe, but advanced hypotheses, in order to construct higher mental phenomena from the lower, or to reduce those to the latter. Everyone who reads these accusations, if he knows psychological work through his own participation, will scarcely be able to suppress a smile. For this construction from lower elements, which Dilthey criticizes, becomes possible only after these latter have been discovered by analysis; and that this analysis was neglected by the psychologists of the last decade of the nineteenth century even the most prejudiced opponents of the old psychology will not contend. But Dilthey holds the erroneous opinion that it is the special province of psychology to create a foundation for mental sciences, i.e., sciences which are generally concerned with very complex and individually shaded facts. Psychology, as a general science, has of course the duty to investigate individual psychic facts in this line, and must not exclude applied psychology; but its main task must be the generic determination of psychic facts and laws. Psychology has her own scientific aims, and serves other sciences only in the second place. Here again, Dilthey is mistaken, as he believes that this, his favored psychology, which is to create a foundation for mental science, may be distinguished by its describing and analyzing methods; for the individual psychologist not only analyzes and describes, but also unites and combines, and constructs complexes, as the excellent historical works of Dilthey himself very clearly show. On the other hand, natural science dispenses, by no means, with description and analysis. On all these points, Dilthey's presentation is exceeded by far by the well-known one by Rickert. At any rate, according to my opinion it is certain that psychology must proceed describing and constructively analyzing and synthesizing; and, like every science, cannot dispense with hypotheses. Also there is no right nor reason in Dilthey's endeavor to separate psychologists into two parties.

Today the psychology of natural science is often contrasted with the psychology of the *Geisteswissenschaften*. This contrast, which is practically the same as Dilthey's distinction, has been artificially exaggerated. If it refers to methods, it is self-evident, of course, that psychology uses those of natural science (experimental, etc.), as well as those of mental science (see above). With the latter I mean especially the *Sich-Hineindenken* (*"Eindenkung"*) and the *Sich-Hineinfühlen* ("empathy"), which I shall discuss further on. The fact is that the advocates of this contraposition understand by "natural-science" psychology something much more special, i.e., a psychology which borrows her methods and principles from the mathematical natural sciences. With what right can we then speak simply of "natural-science" psychology and thus create the erroneous impression of a sharper generic contrast between all the natural sciences and mental sciences? Besides, there is no reason why every psychologist should not use also mathematical methods, of course, within the limits of their applicability.

Special variations of the latter contraposition are produced if we introduce the conceptions: relation to a unity (whole, also designated by the monstrous word *Ganzheitsbezogenheit*), disposition, purpose, structure, sense (significance or meaning), understanding, generally, of course, without a clear definition or delineation. In the first place, concerning the relations to a united whole, these, of course, have never been denied. These do not appear first in organic life, not to speak of psychic life, but without any doubt are also contained, for instance, in a crystal. We may even claim that every partial differential quotient represents in the abstract such a "totum-relation" (*totum Beziehung*). That such "totum-relations" gain ever more significance in the organic field and still more in the psychic field, we can admit without hesitation. Therefore, we may rejoice that they are noticed and studied more and more everywhere. Only we must be careful in their interpretation and not smuggle in all kinds of metaphysics. According to my conception, they are the relations between the parts and the total complex combined to a unity. From relation and complex (united aggregate) the fact of the "totum-relations" becomes quite clear. The total of all these relations within a complex is its structure. It meant indeed great progress when Mach, and later Ehrenfels, by means of the so-called *Transponierbarkeit* (transposability—for instance, of a melody), demonstrated the importance of the relations, and thus opened the way for the theory of the *Gestalten* (or rather *Komplexionen*). However, it seems little to the point to inscribe in capital letters the word *Structurpsychologie* or *Gestaltpsychologie* on a party flag and prate about it everywhere. It would seem much more important to find new facts and laws in this field in order to understand those already known better and more "unitedly." What great significance I myself ascribe to these *Gestalten* is shown not only in all recent editions of my textbook, but also in my *Aesthetik* and *Logik* and in many shorter treatises and essays.

The above-mentioned structures very frequently are not directly effective, but require further inner development or some outside factor in order to become effective. They are then called "dispositions." Here again some psychologists adopt at once the term "disposition psychology" and assume a hostile attitude towards psychologists who do not recognize the "disposition conception" as the only soul-saving creed for the entire psychology. This wrangling about slogans seems to me quite superfluous. Therefore, I followed with profound regret the recent controversy about complex theory and *Gestalttheorie;* dispositional impregnations were also drawn into the discussion. The actual problem evidently is to decide, through the discovery of new facts, between the two interpretations in certain divisions of psychology (a task that I have been working on for some time). Why, then, two hostile army camps within the realm of psychology?

I consider Spranger's counter theory of sense-free and sense-bound psychology especially dangerous. It is defined thus: That has sense which is fitted as a constituent part into a value-total (*Wertganzes*). I have rarely read such an insufficient definition, and I can show that even Spranger did not always verify this definition. The German word *sinnvoll* may be interpreted very differently, for instance, as "valuable" or as "suitable" or as "conceivable by a united conception complex," or (as, for instance, in the semantic of words and signs) as that which may be replaced in my thoughts by a definite coordinate conception; but whatever definition one may select, I consider the word *Sinn* as totally unfit for a fundamental distinction of two opposed main (directing) rules to be used in all psychology. The fact is that with many authors the word "sense" has already degenerated to an empty, meaningless word.

Like every psychologist, I demand an *understanding* psychology in the general sense. Without understanding, no science. But the disciples of the highly praised understanding psychology evidently mean by "understanding" a very special kind of conception for which, among other things, empathy is a *conditio sine qua non.* That there is such an understanding is true. But it is logically unsound, and terminologically misleading, to speak of an understanding psychology, meaning only one kind of understanding. Overlooking this error, we can admit that this empathy is most helpful, sometimes even indispensable, for the understanding of historical persons, dramatic and epic characters, or even contemporaries (in the case of a teacher or a doctor with patients in mental or nervous diseases). But this empathy is no mysterious activity, or even a deductive process, or anything like that, but simply the well-known process of "putting yourself in his place" (terminologically more correctly *Hineindenken* and *Hineinfühlen*), which I have discussed quite fully in my *Aesthetik* (Part 2, Lecture 3). Just as I imagine my own inner and outer experience and activity in case of an occurrence, so I imagine, for instance, in empathy with Socrates, my own inner and outer behavior in the position of Socrates; but, while in the first case the purpose of my imagining is at the most a preparation for a future situation, in the second case by my imagining I can hypothetically supply many mental processes of Socrates which have not been historically transmitted, and thus experience the general combination of his motives by transferring them to myself. Thus an understanding of Socrates may be developed. The danger of error is naturally very great, as is shown by the differences of different writers in judging historical persons. There certainly is no cause for forcing the ambiguous and false dilemma of sense-free or sense-bound psychology upon scientific psychology.

Much more important is the principle which we may call phenomenological. This should in fact dominate the entire psychology, and formerly did lead psychology in many main points. I am not thinking of the phenomenology that K. Chr. F. Krause and Bolzano advanced, and Husserl upholds even now, which is said to be entirely separated from psychology; I have in mind the phenomenological direction which I formulate as follows: In every psychological investigation, determine first of all the facts pure and simple, i.e., add nothing to them and think nothing of them, and consider these facts as the foundation upon which you are to build and construct. We admit that it is often difficult to follow these directions in full measure. That does not impair the practical significance: the formula indicates the direction we are to follow. Self-evident as this phenomenal procedure seems, still even today it is rarely carried out properly; generally the phenomena as such are slighted and their description is already filled with all kinds of theoretical suggestions or imaginings. The just-outlined direction is quite independent from Stumpf's distinguishing between phenomena and psychic functions. The difference between mechanical and phenomenal physics shows, however, a certain analogy.

I believe that I have outlined my position in regard to some fundamental problems of the present psychology as far as it is possible within the limits of such an essay. To predict the future development of this science seems futile; I can only express the wish that the exact determination of facts and of their connection will again take the lead, in opposition to premature theories, empty names, and slogans. Personally I should like to express the hope that, without the least detriment to applied psychology, we might remember also the relation to the theory of understanding in the sense of a *prima philosophia.*

H. ZWAARDEMAKER

AN INTELLECTUAL HISTORY OF A PHYSIOLOGIST WITH PSYCHOLOGICAL ASPIRATIONS

The reader who by chance looks at these pages will be disappointed at finding the intellectual history of a physiologist, of a *scrutator naturae,* when he expects to find that of a psychologist, of a *scrutator mentis.* But in the days of old, before Wundt, when psychology was not yet an independent science with its own institutes, its own periodicals, its own congresses, almost all students of the honorable science of the human mind were either philosophers or physiologists. Moreover, a physiologist, though there is much to criticize in him, is an observer of man, of the feeling, emotional, intellectual, doing man. Therefore, I have given up my initial fear and will try to trace the steps by which, in the days between 1875 and 1885, a young man became interested in the beautiful psychological sphere—which was to remain to him a promised country—and to describe how he sees its future now.

In my case, then, I started from medicine.[1] Histology, bacteriology, experimental physiology, and pathology formed the school through which I passed. Some original work was finished in those days and in these fields, but I will not treat of it. I only mention it to show that it was not a systematic education which brought me to psychology; it was curiosity concerning mental things, and nothing else. Methodically, I was guided by the strong rules that are common to all experimental sciences; these rules alone permitted me to find some new facts in the leisure hours remaining in the midst of the responsible work of a physician in a leading position.

The Analogies between the Sense of Olfaction and the Sense of Sight. My first step in psychology was an essay dealing with the analogy between the mode of mixing odors and the mode of mixing colors, for I suppose a strong resemblance between the two senses in this respect. In both, external energy excites the sense cells in a most direct manner. The light passes through the transparent tissues of the eye and penetrates to the cells of a projected part of the cerebrum, the retina, where the stimulation takes a new form, that of nervous function. The air loaded with odor passes through narrow paths to the utmost roof of the nasal cavity. There the olfactory cells are distributed, again closely connected with another projected part of the cerebrum, the bulbus olfactorius.[2] Is it now permitted to conclude the probability of analogy from an apparent homology? And, if this is permitted, does the analogy exist?

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[1] Born in a literary social sphere (my father was a bibliographer, my mother a novelist), I excelled in my boyhood, through certain accidental combinations of qualities (for nothing is known of mendelian influence), in mathematics and science. For that reason my parents planned for me a medical career, hoping that I would direct my interests into a broad field. After completing the university curriculum, I became an army surgeon, in 1882. In that capacity, I served at many posts, always in large hospitals. At the age of 40, I succeeded Engelmann in the chair of physiology at Utrecht University, Engelmann leaving Utrecht for Berlin in 1897, where he took the chair of du Bois Reymond.

In the two senses, sight and smell, the variety of qualities of impression which come to us is infinite. Nothwithstanding this, Thomas Young had reduced the variety of effect in color feeling to three fundamental kinds of stimulation, and the study of contrasts and after-images had completed the system, supposing these to be an interaction of nervous processes.[3] Should it be the same with olfaction? To draw a conclusion about this, it was necessary to treat, first, the primary excitations and, secondly, the concomitant nervous processes.

It seemed practicable to begin with the physical side of olfaction. This touches the bottom of the problem, and all work devoted to the so-called *Erleben einer Geruchsempfindung* is idle when the peripheral excitations are not isolated and cannot be reproduced exactly in quality and intensity.

And so it happened that in 1884, in my experimenting room in the Military Hospital of Amsterdam, I asked Nature which are our principal and most elementary smell sensations and why do they influence our higher reflexes or how do we become conscious of them? But Nature did not answer. The difficulties appeared insurmountable, for I had not devised technical means of avoiding the absorption of odors by the walls of the receptacles and the tubes I had to use, when I wished to isolate and mix my odors. I had to stop the investigation till a more suitable time.

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[2] Later the homology between the two sensorial organs appeared to be still greater, because, in both, the transition from external energy to the vital process takes place in nervous cells or in the immediate appendix of them.

[3] It is curious that Hering, in his color theory, also started from an analogy that was supposed by him to exist between the sense of temperature and the sense of sight. It is a pity that this analogy could not keep ground against the facts discovered later on by Blix.

Two years later I was ordered to the High School of Veterinary Medicine in Utrecht as a temporary lecturer in physiology, and just at that time it happened that a memorial volume was being prepared in honor of F. C. Donders' seventieth birthday. In September, 1887, while I was working in my small laboratory, an invitation reached me to deliver a contribution to that volume, and while I was meditating on the choice of a subject, the memory of the long-forgotten problem of the analogy between odors and colors came back to me. Moreover a new resemblance occurred to me: Olfaction is the sense upon which thinking is based in the higher animals, while vision assumes the rôle in human beings. Would the technical means at my disposal now be better than were those in 1884?

As I thought over this question, I took in my hand a glass tube and a piece of rubber, with which to join tubes, and, thoughtlessly slipping the one over the other, I suddenly had the inspiration that we, in that manner, had at hand a method of measuring the odorous sense, and, through that, the sensory value of any given odor. The laboratory assistant was immediately called. I gave an order, first, to cut a piece, 10 cm. in length, out of the middle of a new gas tube; secondly, to take a glass tube of 15 cm. length and to curve it up at one end; thirdly, to make a little screen with an opening through which the glass tube could pass and be fixed with a cork. The three simple things were soon ready and put together, and in a quarter of an hour the "olfactometer" was born.

To obtain my own threshold as well as that of my helper, we were obliged to push the rubber 1 cm. forward over the outer end of the glass tube through which we inhaled. After having fatigued ourselves a little for the smell of the rubber, by inhaling at the long tube, we were soon restored so that, after a short time, we could find our old value. I called that value the "olfactie" of rubber, supposing that every solid odorous material, tested in the same manner, would show its own olfactie. During the following days, we constructed many such olfactometers; wax, resins, drugs, woods, etc., served as odorous materials. We always found the threshold-value, given by a certain length of the smelling tubes. On different days the values were the same and each of them characteristic for the substance with which it was established. The time needed to become fatigued, however, varied with different substances and also the time necessary for cleaning the inner tube through which we inhaled. Superficially considered, the tube remained pure, but we learned that odorous molecules adhered in some cases for a very long time through adsorption. As we used our olfactory sense to discover the traces of remaining odor, we had to control our fatiguability; it showed itself to be bilaterial, even if only one side was exposed to the fatiguing stimulus.

Thus, apart from adsorption and fatigue, the principle of slipping one tube into the other can serve the same purpose in the regulation of the intensity of odors as does the slit of the Aubert window in the regulation of light. Only we always have to control the adsorption and to beware of fatigue. Very intense odors should be avoided. For that reason most of the older observations can be accepted only with reservations. Therefore, it is a pity that even in recent days psychologists and clinicians very commonly use pure chemical substances, undiluted, and, if diluted, not controlled. Much time is thus spent in vain.

A few days after the "invention" of the olfactometer, I walked with Donders from his house to the laboratory. Passing along one of the quiet canals that cross the Dutch town, I took the olfactometer out of my pocket and handed it to Donders. He stopped, looked at the little instrument, tried it with slight astonishment, and, returning it to me, said with his warm voice, "That is beautiful, for it is simple," and happily I could reply, "It is useful, too, for we can now mix odors, as you mix colors in your double-slit spectroscope, using odor equations instead of color equations," and then we talked along on the analogies between the two senses.

In the weeks after this discourse, I tried to obtain mixtures of odors in a simple double-olfactometer constructed with solid substances. I wondered that it was so difficult to obtain homogeneous impressions and, further, that such mixtures are always weaker in intensity than the sum of intensities of the components. Even when I balanced the quantities exactly in a convenient proportion, it very often appeared possible to arrive either at a complete absence of odor or at least at a vague sensation without pronounced odorous quality. In other proportions, I found a prominence of one of the two components in such a degree that the other one was completely suppressed. I called the first phenomenon compensation of odors, produced unilaterally or bilaterally; the second phenomenon I considered a struggle of sensations leading to over-compensation. When the compensation (which is best obtained with weak intensities, i.e., of camphor and scatolwood) is not absolute, no distinct smell remains, but only some undefined impression; it is as if we enter a room where we recognize some slight odor without being able to distinguish what smell it is, nor to describe the quality. The sensation is often the same and it is familiar to us; yet we cannot define it. A really odorless air is seldom met with; only in winter we may find it while walking on a field of snow.

Before going further I wished at that time to study the theory of the instrument. We constructed in the laboratory a "tachistoscopic" machine in which an odorous substance with a surface of certain dimensions could be exposed during a short measured time to the current of air which we inhaled, as we did while using the olfactometer. Of course we had to study the mode of inhalation beforehand. When we sniff at the olfactometer, the air enters our nasal cavity through the anterior half of the nostril and passes in a curved stream along the septum, ascending to some extent in the olfactory fissure, or remaining below, this depending on the rapidity of the current. It is obvious that, in smelling, we seek by trial and error the most convenient mode of inhalation to obtain a clear impression of the odor presented to us.

During quiet respiration the curved stream will not ascend higher than the entrance of the fissure and most odorous molecules will remain below, just as in a narrow street the dust which is stirred up by the wind does not reach the higher stories. The respiratory inhalation, however, is not, as with dust, the only force which moves the molecules. Diffusion is the other factor. It helps the small and medium-sized molecules to ascend in the fissure. Only the small ones will reach the roof, the heavier ones remaining beneath. Through considerations of this kind I was led to a theory which localizes the smallest molecules (of course, the first term of a homologous series, fatty acids, ethers, ketones, etc.) in the highest spots, the middle terms in the lowest spots of the mucosa olfactoria; the heaviest terms are odorless, and therefore without sensory location at all. Going a step further, I imagined that the different homologous series were localized, the one before the other, along the path of the inhaled air. It was not necessary to decide in what order the arrangement takes place. Let us hope that, in the future, the experimental study of the olfactory reflexes in animals and in men will reveal this. Such was the origin of my mosaic theory of olfaction.

In order to find out later on, through experimental work, the real topography of the hypothetic mosaic floor, it appeared necessary to study not only the intensities of the sensations but also the qualities. About the latter, we can judge only as to the neighborhood of the threshold. Therefore we constructed an odor box with movable glass walls, which could readily be freed from the adsorbing odor of the preceding experiment. When the investigation had reached this point, the War Department judged that I had occupied my lectureship at the Veterinary High School long enough and called me back to the Hospital to lecture there to the young army surgeons. Hamburger was put in my place. The latter measure was not bad, as the history of physiology has shown, but it was hard on my experimental work. Nevertheless, it brought the advantage that in the Military School in Utrecht I found plenty of men to observe, even pathological cases. Moreover, with the kind aid of the apothecaries I could study the odors of all drugs accumulated in their classic store, and had a big library at hand. We constructed many solid olfactometers. We also made solutions of pure chemical substances in water, glycerine, or pure liquid paraffin, and plunged into them porous tubes of 8-mm. width, with glazed ends. Such imbibed tubes take the place of the solid odorous cylinders in our simple olfactometer. In this manner my experience was growing, and it was no longer audacious to attempt some classification embracing all odors occurring in nature and in technical laboratories. I believed that the whole scheme would be well adapted to the purpose, better than if I had followed Haller, who discerned the odors according to the affects awakened in man. Only I had to avoid every subjective judgment, for it was not the belief of any individual investigator that I had to register, but the impressions of soldiers, boys, and girls, supposing that all men should have the same odorous system, or at least that the number of odor-blind persons is not too large. For that reason I did not ask any learned man about the impression he received from any material in the neighborhood of its threshold, but I let myself be guided by the man in the street, or I consulted literature, in that case always comparing the description with the substance lying on my table.

It is necessary in such a study to arrange the conditions in an ordinary way, for, when anyone perceives that he is a person with whom we are experimenting, his judgment takes a scholastic color. He enters into the mode of thinking of his questioner, with all its fascinations.

Some years passed, and in 1897 Engelmann was called back to his native country, and the Government offered me the Chair of Physiology at Utrecht University. So I became an academic man. I left the Hospital and turned to the laboratory, at the same time to the world of students, graduates, colleagues, coming with their problems and needs to the director of a university laboratory, which had always been a research laboratory, too. So other things than olfactology were to be done. On the other hand, I chose a skilled staff and made use of my opportunity to turn my simple olfactometer into a precision model and to control its time relations more exactly. But, in spite of all these advantages, I soon stumbled against new impediments.

Olfactology. Olfactology can be divided into two parts: (*a*) the peripheral action of odors, stimulating the sensory cells of the olfactory membrane; (*b*) the physiological and psychological sequences (the conditioned reflexes and the sensations). The progress in the first division is highly influenced by the state of natural science. We are completely ignorant as to what the essence of an olfacto-chemical process is, for we lack the model we possess for light in the photographic film. The only judgment we can maintain is the hypothesis that all olfacto-chemical action depends on special atom groups in the molecule. In 1895 I had called these groups "odoriphores." Since then much work has been done in this field. Isomeres, asymmetries, and unsaturated compounds have been studied with the greatest care. The physiologist and the psychologist, however, are handicapped in these matters, for we cannot perform such investigations with the chemical preparations of commerce. We need preparations of great purity, which only the chemical laboratory can procure for us. If kept for some weeks, many of these pure bodies polymerize. We want, therefore, the aid of chemistry, and hence the collaboration of different laboratories; this necessitates the task of organization, which is not an easy one.

When we had become acquainted with the odoriphores, the question arose as to what force was acting in them. It must be a force by which some influence on the receptive substances of the cell can be exercised. Perhaps an infra-red vibration awakes a resonance in the receptive substance, an electron is emitted, and the nervous chain is stimulated. This supposition has some probability, for Tyndall discovered, and Gryns affirmed, a strong absorption of infra-red in the odorous gasses. We wished to control the specific action of the infra-red rays as Heyninx has done with the ultra-violet rays in relation to olfaction, but it was not possible to find a room in the laboratory appropriate for the installation of the delicate instruments we had to use in such an investigation. Again we had to wait for a more propitious time.

After this discouragement another *fata morgana* appeared. Becquerel, the discoverer of radioactivity, spoke one day in a congress at Leyde on emanation, which adheres to the walls of receptacles "as odors do." This remark of the great experimenter made me look for some radiating effect in odors; but in vain, no photographical action was found nor any ionization in the air. In 1915, however, while extending the investigation to odorous nebulae, we discovered a strong electrical charge in the drops constituting the clouds we produced by spraying. The odorous clouds alone bear that strong charge, the inodorous ones bear no charge, or at least one of a much lower degree. We therefore called the new phenomenon the *charge phenomenon* of odorous substances, all odorous substances showing it. It much resembles the waterfall electricity, but it differs from it because pure water in a fresh state gives no electrical charge at all when sprayed. When comparing the odorous power of any substance in aqueous solution to the power of charging a cloud with electricity, we found that both properties disappeared at the same point of dilution. The various odorous solutions differ, however, odors producing a stronger charge than perfumes. The phenomenon is in no way specific to odors, as the following mean values per cc. of sprayed solution demonstrate:

Charge-phenomenon in 10^{-10} Coulomb

Odorous substances	81	(extremes 300 and 1)
Saponines	5	(extremes 16 and 0)
Antiperetics	4	(extremes 7 and 1)
Alcaloids	3	(extremes 3 and 0)

Probably the phenomenon depends on the lowering of the surface-tension of water, brought about by the addition of the dissolved substances. The odorous substances possess the property in the highest degree. So the study of the electrical phenomenon, although non-specific as to odor, made me, nevertheless, consider the olfactory sense as a sense of adsorption, catching all molecules which lower the surface tension. Those molecules which evaporate too quickly and, when in a gaseous state, diffuse too rapidly cannot serve the purpose. This is true for animals living in the free air. But the odorous molecules certainly belong to the molecules seized. Such molecules are carried along with the respiratory air and are held by adsorption at the surface of the olfactory membrane, where the Bowman glands secrete their fluid. Here the molecules are dissolved and the odorous ones, which are all soluble in oil (1916), are transported to the hairs of the olfactory cells. Taken up by the lipoid substance of these hairs, the odoriphoric groups are able to display their olfacto-chemical action. In the receptive substance of the cells the energy is created which transfers the sensory process along the nervous path. The outer stimulus irritates; the receptor provides the energy needed in the neuronic chain.

It is evident that the worker in the physics of olfaction finds his way blocked and has to wait for some new discovery, disclosing a new area in this field. But for this the time must be ripe, otherwise it is impossible to pluck any fruit even from the most tenacious work.

What happens in the other field of exploration, where the propagation along the chain of neurons conducting the impulse from the olfactory membrane up to Ammon's horn takes place?

During the years through which I waited for some revelation in physics which could explain the action of odoriphores in the olfactory cells, I tried to conjecture as to the manner in which the conditioned reflexes and the sensations of smell originated from the stimulation of the receptor as just described. We can study this as Parker and von Fritch did in animals, or as I did with men. Perhaps the first method is the most logical one, for in man olfaction is a lower sense.

We men do not think, as do the osmatic animals, in olfactory images. Our complex visual images, extraordinarily plastic as the consequence of our binocular vision, are completely wanting in osmatic animals. Our complicated acoustical images, passing one after another with a grammatical discipline, images in which we feel the mighty influence of speech, in animals make only a simply shaped impression. It is otherwise with the olfactory sensations. Probably in osmatic animals a world of richly varied beauty is awakened which we do not understand, because our brain is not adapted to such a task. The sense has only kept its acuteness in us because the conditioned reflexes have reserved for it a great importance for the digestive functions. Nevertheless, I have not chosen the comparative method (*a*) because I started from medicine, (*b*) because my pupils were to become physicians, and (*c*) because my co-workers were medical men. Besides these considerations, there was a technical motive. As this latter one will interest the reader, I will treat of it alone.

When an investigator wishes to do any experimental work with animals about whose sensations he cannot judge, because he lacks the organization to feel and to discern these sensations, he should be sure that his object, when not stimulated, is completely free from external irritation. He should be certain, therefore, that he can put his animal into a perfectly odorless room. No such room was available in my laboratory, but in the meanwhile I tried to procure an odorless chamber. The air is said to become odorless when it is pierced by ultra violet rays. Commonly this is explained by the development of ozone but it can also be caused in a direct way. Since 1912, therefore, I have tried to study the phenomenon quantitatively with all the odorous substances at hand. We followed the method of diminishing, in a glass receptacle with a quartz window, the odoriferous power from two olfacties to one, the air within the receptacle being radiated by a mercury quartz lamp. Some odors are refractory to the action of the rays; most of them, however, are destroyed. In these cases we suppose a direct influence, oxidations, syntheses, and polymerizations. If we want to do that, however, we have to exclude the production of ozone, which we can obtain by some technical artifice. So it is possible to recognize the odors which can be removed from any room, even when we wish to give it sufficient dimensions.

We have a simple method of controlling the efficiency of such a box. To this purpose, we slowly suck some of its air through pure, fresh water, and spray the water. If any odor remains in the box, the sprayed water gives the phenomenon of which we spoke (electric charge). This test is probably also binding with respect to the animal sense.

If we wish to become acquainted with the process which takes its way along the neuronic chain from membrane up to Ammon's horn in man, it seems rational to begin with the determination of the perceptible minimum. The simplest method is to evaporate a trace of an aqueous solution of the odorous substance in an odor box with movable glass walls and then to estimate how many fractions of a gram are present in 1 cc. of the air in the box at the moment when the threshold is reached. This value varies greatly. With ionon it is 1.10^{-13} gram per cc. of air. If we wish to express this value in units of energy, we can transfer it to heat when the substance is burned. But only a very minute part of this heat figures as free energy in action in the olfactory cell. Unfortunately we cannot calculate the proportion between the two values, combustion heat and sensory energy.

Now that we have discussed the threshold, we have to follow the nervous stimulation on its way along the neuronic chain. When, in short, I leave the time relations aside, we meet with the phenomenon of fatigue. Perhaps from general information about inborn and conditioned reflexes something can be deduced on this point.

About the refractory time of reflexes in general, we have gathered some facts. It was in 1896 at the Military Hospital that I observed in man a cutaneous reflex with an extraordinarily long refractory phase, measuring some seconds. A year later Richet and Broca found that in the dog such a refractory phase measures .5 second. With Lans, I discovered in 1899 in the corneal reflex a refractory phase of about .5 second. In 1904, in the swallowing reflex, a somewhat longer phase was found. Especially in the latter case, by the application of some subsequent series of stimulations, we found an apparent fatigue of the reflex system, revealing itself in the prolongation of the refractory phase. Since we lack accurate experiments about the conditioned reflexes evoked by olfactory stimulations, it is too hazardous to suppose from general information that fatigue is essentially founded on a lengthening of the refractory period in the olfactory reflexes. But in conscious olfaction this may be the case, for olfactory fatigue is principally localized in the neuron chains. We can derive that fact from the bilateral appearance of fatigue if unilaterally evoked. Perhaps a peripheral fatigue exists, too, as Backman supposes, but the synaptic fatigue dominates. Through this fatigue the sensitivity of the sense is always somewhat decreased during the moment following that of excitation, as is well known to experts who taste tea or wine; and the duration of that blunted phase increases with the intensity of stimulation. Considered in this manner, olfactory fatigue, in literature described also as a form of adaptation, is the analogy of the fatigue of reflex action in general.

Only one objection can be made. With the artificial swallowing reflex of the cat, the refractory phase is topographically bound to the motor part of the reflex act, as I found in 1904 and Sherrington confirmed later on. In the conscious reaction, the motor part is wanting, or at least is inhibited. It is obvious: *omnis comparatio claudicat!*

When we do not restrict ourselves to single stimulations, but also consider combinations, the final result in sensation is determined not only by the intensities but also by the qualities used. The special study of these relations by means of introspection is in olfactology peculiarly difficult, because observers are generally not accustomed to distinguishing accurately the qualities and intensities of the smells they meet. Usually they do not distinguish synchronous, subsequent, and alternating sensations. The best results are obtained with culinary sensations, when these are received by gustatory smell. With perfumes it is not so easy, especially when the observers introduce their preferences into their judgments. Some discover chemical resemblances, others are strongly impressed by the agreeable or disagreeable effects connected with the sensations.

Under these circumstances I tried to get some unprejudiced and unaffected information by means of a zero-method, combining two odors of different qualities in a double olfactometer with an inhalation tube in the shape of a T. If the intensities are not too great, we can generally obtain odorlessness, or the vague, undefined impression of which we have spoken above. In that case all subjectiivity has disappeared, for there is neither preference nor affect. All observations have the same color. A simple physical experiment is performed and no qualitative description is asked from the observer. The zero-method of compensation takes the place of the method of a psychological *Erleben*. Moreover, we have the possibility of a mathematical representation. The two sensations can be imagined as two vectors representing two forces counterbalancing each other in our intellect. We can combine them into a parallelogram of forces by addition of a vector of odorlessness.

Such representations we can construct with all combinations of odors tried which led to compensations. We can, however, seldom add a third vector in space, the system being founded on three proportions. I succeeded only once with 252 combinations: when I combined terpineol, ethylbisulfide, and guajacol. The result was discouraging, and I did not continue this line of research. Not that I doubted that some psychological system must exist with well chosen odors in which three odors can be represented at the same time with regard to the proportions found by compensation, two by two, but the determinations seemed to me to be deficient. The dilution of the odorous substances should have been greater, either in water or in glass powder, if we prefer odorimetric tubes of solid materials. The adsorptions should be avoided, the glass receptacle of the precision olfactometer better cleaned, our odors better chosen. It seems to be possible to arrange these improvements, but I lacked time, and the vectorial investigations remained unfinished.

I returned to the simple study of the proportions in which the compensations take place. It seemed, for instance, interesting to determine in how many cases the proportion, given in olfacties, was the same for three skilled experimenters. It appeared in 15 out of 252 permutations. Discouraging again, probably for the same reasons as were present with the vectorial representations.

Such failures do not diminish the merit of the vectorial representation. The rationality of representing any odorous sensation by a line in space remains undisputed, for the direction can correspond with the quality, giving a straight line, if the substance maintains the same odor from the concentrated to the diluted state, and giving a curved line if the quality is variable with the concentration, while the length simply measures the intensity in olfacties. But the experimental determination of the properties of a vector, direction and length, was in my case not sufficiently perfected.

The carrying out of improvements depends on the occasions offered and on the character of the investigator. In the latter respect, I have failed. The only excuse for me is that I had at the same time a heavy educational task, and the improvements would, moreover, have taken more time from the technical laboratory assistants than the work of the graduate students could allow.

I tried still another method, that of fatiguing the sense with any pure odor and testing in the subsequent moments the sensitivity of the sense.

Let us suppose, so as not to contradict Backman, that fatigue is universal, originating as well in the olfactory membrane as in the synapse. It is probable that such a universal influence does not occur everywhere at the same time, neither has it the same duration. We therefore take fatigue as a whole and do not occupy ourselves with its localization. For the same reason it is a matter of indifference to us for the moment if the experimenter prefers to speak of adaption or of fatigue.

In such an investigation, with the exclusive use of pure chemical substances, we can discern a homonymous and a heteronymous fatigue. The first one we meet with when the fatiguing and the testing substances are the same, the second if they are different. It is curious that if we carry out the experiments with a great number of odors we find that every odor tried leaves behind some slight heteronymous fatigue besides a strong homonymous one. We conclude that the olfactory sense must possess something which is common to all qualities. Perhaps it is the vague, undefined impression, already discussed, which remains when a set of compensating odors is not completely balanced. Its significance is very uncertain. In fatigue experiments, we can show its appearance easily if we use a large odor box with an olfactometer in its floor, put our heads into the box, fatigue ourselves with any odorous air stored in it, and measure the sense before and after fatiguing. The loss is very slight but never wanting.

The choice of fatiguing odors, which we store up in measured concentrations in the box, is somewhat difficult, for it was necessary to avoid intoxications. In this regard it is obviously better to use substances which occur in daily life instead of pure substances with unknown toxic level.

Later on we extended the investigation of heteronymous fatigue, principally to that for mutually related odors belonging to the same Linnean class. We could then judge the relationship from the loss of sensitivity after fatiguing. For the aromatic class this has been performed, the other classes are yet waiting. I believe that by following this method we can build up a natural system of odorous sensations in man and, by the means of conditioned reflexes, also in animals. Such an inquiry, however, takes much time and is very laborious; but as a quantitative study, it leads, when the measurement is sufficiently accurate, to a great certitude.

The Sound-Proof Room. For the same reasons that I wished for an odor-proof room, I desired to acquire the use of a perfectly silent room. Therefore in 1904 when the laboratory had to be somewhat enlarged, we took occasion to construct a *camera silenta* in the upper story.

I was guided in my attempt by a remark of the French physiologist Charpentier. He had to make threshold estimations in tonal sensitivity, and for this purpose used a room in an insane asylum at Nancy. At that time padded cells were used in the asylums. Charpentier preferred such an experimental room, because the reverberation in it is slight, and the acoustical reflection on the walls is very slight. Following out the idea of the padded cell, we covered the walls of our silent room with absorbing material borrowed from a Belgian publication, Dr. Biltris recommending a thick layer of horsehair as an excellent covering for telephone cells.

The dimensions of the whole room were planned to be no larger than strictly necessary (2.2 meters square) ; we hoped thereby to reduce the invisible leaks which will always remain in such construction. The room was separated from the outer walls and the roof of the building by expressly arranged non-reverberating spaces and was moreover surrounded on all sides by quiet rooms, such as a photographic chamber, a storeroom, etc. To further prevent the entrance of noises we made double walls with various somewhat porous materials (porous stone, corkstone, etc.) and an air-space (one inch wide) between them. No contacts were allowed between the two walls except well-placed lead blocks. Double doors and windows were made, and openings (1 cm. in width) for the entrance of acoustical signals were arranged, but could be shut with soft rubber stops if not used. The very slight porousness of the double walls was chosen, hoping that we might not need an artificial ventilating system.

When the room was ready, we were at first content, for threshold measurements could be well made. In the silent room most of us heard a *tinnitus aurium*. It had a rushing character and was often accompanied by a very high-pitched whistling. Ordinarily the tinnitus is continuous. Only the technical assistants, when they had done some heavy work, indicated a beating form. Inexperienced young men, such as the students, sometimes missed the tinnitus and perceived in its place an increasing feeling of pressure in the ears. When an observer was in the room for a long time, for instance, three hours, the tinnitus completely disappeared; but if he left the room for a short time and then returned to it, the tinnitus reappeared.

The silence in our chamber was not absolute. Some deep noises conducted by the floor remained, principally those caused by heavy cars passing in the street. Therefore, we installed in the chamber a movable case with turf walls, covered inside and outside by thick layers of horsehair and suspended by metal springs from pillars, placed on the floor which was covered with horsehair and a carpet. These pillars rested on anti-vibrating blocks, and these, in turn, rested on multiple layers of various substances of different density.

This density was measured, for it was obvious that, when the substances through which the noise must pass vary in density, most of the sound will be reflected and only a small part of that which remains will be conducted. When the observer was sitting in the inner suspended box, which we called the *camera silentissima,* the result was excellent. When all doors were shut no noises were heard.

The sensation of such a complete silence is very curious. It is necessary to leave one's watch outside the chamber. After-images of sudden strong tones can be observed. In shells of various dimensions every tone, which is a resonance phenomenon, is wanting, and so on. But immediately upon entrance to the room a weak tinnitus in the ears begins. It resembles the wind in the tops of the trees in the woods. Besides this, a high tone appears which is very near the upper limit of hearing along the tone-scale; also more complex noises occur, which take on, in different degrees, the character of hallucinations, resembling the singing of birds, cock-crowing, melodies, etc. Perhaps these sensations are essentially illusions, originating in some circulatory sound which is inaudible in our ordinary surrounding, but which become audible in this silent environment. It may, however, also be that some of the subjective noises are after-images of what was heard before in the street, in the workshop, etc. This supposition seems probable, since the inner noises disappear after a time and come back if we return to the *camera silentissima* after a short time spent out of the chamber. Nevertheless, under these cirsumstances the circulation noises increase and decrease, so that the other explanation also is tenable.

Pavlov constructed silent rooms of this type for his dogs, and many otological clinics have arranged similar rooms of greater dimensions. An extraordinarily good sound-proof room with three complex walls may be found in Frankfort a/M. on the first story of the hospital building. The University of Utrecht had installed one under the out-patient ward. In the latter case, it was necessary to place five cages, one inside the other, with air layers between them. The result was excellent, but the costs are too high to make this plan feasible for general use.

The cheapest place for the construction of a silent room is in the upper stories of a building, but if the chamber is placed there, we must remember that mirror instruments can never be completely stable when the whole house is moved by the wind. On the other hand, the slow vibrations coming from the floor are better avoided upstairs than below. As the purpose of the chamber is an acoustical one, this consideration must dominate.

Another technical question is that of the materials to be chosen. This depends on the noises which are most disturbing. In the noise of the street we find with resonators that there are principally two distinctly separated tonal zones. The higher zone lies in the part of the scale which corresponds to the speech zone; this is an expression of the human voices resounding in the street. The lower zone corresponds to the sounds made by heavy carriages. The former are best avoided with substances like porous stone, cork, turf, celotex, the latter with lead and very dense substances. Therefore, the first substances should be used in the construction of walls, lead in the construction of the floor.

The ventilation problem must also be considered in the choice of material. A small experimental room, as that of a laboratory, can be without any special ventilation if the walls are somewhat porous, but the large chamber used for clinical observations needs particular installations to supply fresh air. Ventilation canals are required which introduce *eo ipso* noise from the exterior. For that reason, in the course of the canals a certain number of small intervening cellars with covered walls (e.g., turf covering) must be introduced, which makes the construction a very complicated one.

The number of investigations which are facilitated by the accommodation of the sound-proof room is exceedingly great. In our chamber studies have been made on the following problems: the changes in tones and noises caused by narrow tubes, the sound conducted from ear to ear, the conduction of a speaker's or a singer's voice through his head, the sensation of a clap at the beginning and at the end of a tone (spoken explosivae if the implosion and the explosion are unnoticeable), the reverberation, the resonance in shells, the reflexes on the walls, the deadening of sound by materials depending on the dimensions and the fixation on underlying material, and so on.

The Threshold of Sound through the Scale of Pitch. One of the most interesting investigations which can be executed in the silent chamber is the determination of the threshold values of sound through the range of tones to which the human ear is sensitive. This range extends from 10 d.v. to 23,000 d.v. During our whole life the *a d'orchestre* (435 d.v.) lies in the midst of all tones, even when the range is shortened both at the lower and at the upper end (my *presbyacusis* law of 1890.)

Before we possessed the chamber, I had, with Quix, measured the thresholds through the whole scale at the same time that Wien had done it at Danzig. It was found both here and in Germany that there was a maximum of sensitiveness in the fourth strated octave (c^4). Moreover, we found a sufficiently sensitive zone between g^1 and g^5. From these points, up to the limits of the audible scale, the sensitiveness rapidly decreased. So far the two parallel investigations agreed, but the absolute intensities were extraordinarily divergent. We were in reasonable conformity with Topler and Boltzmann, with Lord Rayleigh, with Wead, but M. Wien found a sensitivity 10,000 times greater than ours. We believed that his result could be attributed to the bone conduction from his telephones to the inner ear, the influence of which he had not calculated. Though that explanation was not improbable (for telephones produce a heavy bone conduction), we resolved to repeat the measurements with organ pipes of known intensity. Organ pipes of the Bordun-register, if they are constructed for tones of a weak intensity, give nearly pure tones. They can easily be tuned by an organbuilder and are transportable. After my fellow workers had tried a series out in the country on windless nights, we took the pipes into the chamber and measured again; for both series the consumption of energy was calculated from the transport of air and from pressure. At last the efficiency was estimated by the application of the famous principle of Lord Rayleigh. This principle in its original form cannot be used for that purpose, but after some modifications (not putting the Rayleigh disk in a resonator, but as Lebedew had done, in the free air, in my case conducting the sound to it through a canal of the dimension of our auditory meatus), it worked out satisfactorily. We measured, in 1905, a new series of threshold values which agreed tolerably well with the former range, and I believe that this range represents the true thresholds. The recent American values of Fletcher, Wegel, and Kranz are not very distant from our old measurements.

Usually the threshold of sound is calculated in terms of the energy per second striking a surface of 1 sq. cm., but, if we control the number of the vibrations sufficient to give an impression of an easily recognized tone in the silent chamber (two in the scale between C and g^4), and estimate the surface of the typmanum at 1/3 sq. cm., we can calculate exactly the minimum number perceptible to our human ear. We found the point of maximum activity to be 1.10^{-10} erg. In the inner ear this small quantity of energy awakes a resonance in a fiber of the basilar membrane. In its turn, the resonance excites some hair cell, and the cell passes over the excitation to the dendrite of some neuron of the ganglion of Corti. The nervous energy which originates here is propagated along the chain of neurons which conducts to the auditory cortical center.

In such a chain of nervous fibres and nervous cells, with the joining synapses between them, no phenomenon of greater frequency than 1000 per second can be propagated. A more frequent one cannot pass, for the refractory phase of a nerve is of the order of 1/1000 of a second, and the refractory period of a synapse is, as we have seen, much longer. Therefore, even if the resonance of the Corti fiber could be transferred into an acoustical energy (which is absurd from a histological point of view, as the hair cell has no stretched parts, and without elasticity there can be no sound), the stimulus as such cannot pass if the original tone belongs to the descant tones we hear in our vowels. This consideration makes it unimaginable that the vibrations of the sound stimulus should penetrate deeper into the sensorial organ than to the Corti fibers. In what form the exciting energy is given to the hair cell by these fibers we do not know. Perhaps it takes the form of an acoustical pressure (1905). Under this influence the excitation is delivered to the nervous fiber, where it takes any nervous form. The legend, formerly believed by scientists, that tones can be propagated by a nervous path "not of course in a gross mechanical sense, but with preservation of period" has to be abandoned since the discoveries of Gotch and Keith Lucas.

Given a value of 1.10^{-10}erg for the external minimum perceptible, the problem arises as to what part of this small quantity is delivered to the receptor. Unfortunately, for the moment that problem is insoluble. But we can attack another, and compare the quantity with that of other senses. For that reason Gryns and Noyons occupied themselves in tracing out the smallest quantity of light passing through the pupil at the moment that, in absolute darkness, a very short and feeble flash is seen. They found the measure to be 1.10^{-10}erg. At the same moment Gryns exclaimed: "Perhaps all senses have the same limit, the limit of the sensitiveness of living substance!" We did not know then that Lord Rayleigh also had a similar idea, but had not generalized it for all senses. He compared only the visual and auditory senses. In both he found the same order. If he had been a physiologist, undoubtedly he would not have recoiled from the broader generalization, for at that time the quantum-theory was not yet a stumbling-block. Moreover, it is problematic even now whether or not this theory should be an obstacle to comparing in ergs rather than in quanta.

To me, however, the generalization of my friend was the motive to measure the static sense. We did it in man and also, by means of observing the reflexes, in animals. With two different methods, nearly the same values were found for the minimum perceptible acceleration when a man on a rotating disk obtains a minimum sensation. The mean value was 1.10^{-7} erg as a primary value. The proportion in which it is delivered to the receptor remains unknown.

In the three senses the stimuli were not identical. With sight it is an energy, light; with sound it is probably a force, acoustical pressure; with the rotation sense a mechanical quantity, a shock. Under these circumstances, we desired to determine more correctly the different kinds of stimuli which are received by our organs of sense.

In science it is customary to look, in such a case, for the physical dimensions (length, mass, time) in which the factors we study can be expressed. When we do that we find the most different formulae: the dimensions of an energy (l^2mt^{-2}) as in sight and in the sense of temperature; the dimensions of a force (lmt^{-2}) as in acoustical pressure or as in the tactile sense and in the proprioceptive system; the dimensions of mass (m) as in taste, and so on. What we call the stimulus, therefore, must be essentially different in the various cases. However, when we penetrate deeper into the problem, it may be that the so-called stimuli are the intermediary agents and that the essential thing absorbed by the receptor is an energy. The generalization of Gryns supposes that. It is only allowable to compare the different senses from the same point of view if we adopt an identity in matter of dimensions. Objects with different physical dimensions cannot be compared at all.

Energetics in Physiology. Conducted thus along a labyrinthine path to a problem which is of a purely physical and energetic character, definitely requiring the application of thermodynamics, stimulated also by the work of the other Dutch physiologists and the repeated discussions with them, I found myself forced to make a broader study of the subject. Even to project a scheme of an energetic physiology seemed to me a demand of the times and a realization not beyond the forces of an academic teacher who lived amidst the founders of the physical and chemical sciences of the nineteenth century. Starting from the receptive side, I occupied myself no less with the active side, the behavior of man, which is still more fascinating, as it leads to that equilibrium with the environment which is, for an *external* point of view, the final purpose of all living creatures. But in the internal body a harmonic self-regulation governs everywhere. It should be possible to reduce the external stimuli and their action as well as their inner relation to the eternal physical and chemical laws reigning inside and outside of the organism; only the proper principles of life, the automatism, the irritability, the memory, must be added. All these revelations should be defined, if possible understood, and, so far as science reaches, interpreted. A heavy task, too extensive for one man, but happily I was aided by numerous fellow-workers on whom I now look back with tender affection. I wrote a treatise on physiology, which went through three editions in my country, the last in 1921, which has, therefore, now been altered. Moreover, time passed by. New doctrines came up. Colloidal chemistry began to dominate in biology, and the principles which the ultramicroscope demonstrates and the colloidal experiments bring to light are not easy to treat in a thermodynamic way. Also the many specific properties, which were revealed by pure chemicals, put their stamp on the new era. How would it have been possible to withdraw myself from all these influences? Perhaps a private worker can obstinately continue on his path, but this is not allowed in a teacher who lives with his pupils and graduate students in a laboratory, which is an open house, situated not in a corner of the world but on a crossing where a lot of travellers pass to and from the continent, to and from the Latin and the Scandinavian lands. I was pressed forward from general energetics to chemistry and obliged to yield. But, forced to follow the spirit of the time, I wished to attack the chemical problem at its root. That root is the atom physiology, which had in former days inspired our predecessors, such as Ludwig, Richet, and others. Meanwhile, however, the ideas about the aspect of an atom were completely changed. Radioactivity had made its entrance into science. Ought it to be excluded from the living organism? I noticed with respect to the diverse kinds of atoms constituting the body (almost not exceeding the dozen) that potassium is the only radioactive atom in plant and animal. It is never wanting. I asked myself whether its feeble radioactivity could be the cause of action in the living organs. The study of some automatic functions revealed to me that the feeble radioactivity in potassium is really the essential factor. This discovery turned me off from mental things. I had to give ten years of my life and more to it. Towards psychology, however, I can supply an excuse. The synapses belong to the automatic organs in which radioactivity reigns; for the corpuscular radiation of radium from without is able to substitute the potassium in nerves, when the material potassium is removed from the synapse, and if, for that reason, function has ceased. Under these artificial circumstances radioactivity brings complete revival. Under natural circumstances potassium does the same thing and then maintains the revived function until death. In relation to bio-radioactivity, only one remark may still be made, for it has a methodological significance.

A problem is not really solved if it is not treated quantitatively. This is an old experience. Harvey was guided by it when he discovered the circulation of blood. Now, when we calculate the number of molecules in our tissues which can be activated by the radiation of potassium, the number seems insufficient to explain the automatism. In each gram of tissue an electron is emitted by 5 of its potassium atoms every second. Now, along the whole path pierced by the electron, 4000 transmissions of energy take place, each corresponding to $0.6.\ 10^{-10}$ erg, or 20,000 per second. But most of these transmissions attack molecules of water, for these are the most numerous. Only a few of the specific molecules are activated. Under such circumstances a chemically directed intellect in modern times supposes an enzyme. Being a physically directed intellect, however, I sought for an autocatalytic relation. A somewhat complicated series of experiments was arranged and, curiously enough, Nature answered. An autocatalytic process is present, and explains the apparent disaccordance between the feeble force of the radiation and the functional results. In this manner the theory of bio-radioctivity with automatin as a chemical link between physical action and function was saved. But now I will irrevocably leave physiology so far as these pages are concerned.

The Energetics of Psychology? Only for the monist can a psychological energy exist, and what he understands by this name should have the dimensions $(l^{2}mt^{-2})$. If he accepts this formula, he can suppose that there is some dissipation of his energy, which reappears in the form of heat. Moreover, he can also suppose some measurable quantity of free energy, which takes the electric form in the nervous centers as well as in the conducting nerve. He can seek for these energies with the compensation calorimeter, when there is a question of heat; with the string galvanometer, when there is a question of electricity. However, no trace has ever been found which was not of somatic origin. We can, therefore, talk about a psychical energy, but we cannot experiment with it.

In recent days the public has become acquainted with the perspicuous models by which we imagine the atoms and molecules enveloped in hypothetical scales built up of circling electrons. The latter electrons belong to the atomic systems, but are borrowed from these systems, when the atoms are united to molecules. About the colloidal complexes, which represent conglomeration of molecules with surface tensions, nothing is known. In this relation now and then the supposition is made (not in physiology, but elsewhere) that one could meet with free electrons in the tissues. Such is only possible at certain points, as when calcium or iron atoms are struck by X-rays or the rays of radium, further, in the neighborhood of some of the potassium atoms. Only the latter phenomenon is realized under normal conditions in ordinary life. The ultra-violet light of the sun does not penetrate deeper than the most superficial layers of our skin and for that reason cannot bring any change in the aspect of the inner tissues. Under these conditions we cannot reckon with the possibility of currents of electrons, as is sometimes supposed by people who are not well-informed. Psychology, of course, can expect nothing from currents of free electrons, and even the monist cannot try to bring any peculiar form of energy in relation to the energetics of electrons, if he does not wish to lose himself in a speculation on a pananimistic influence of the 0.00034 $\mu\ \mu$ rays.

The question takes on a completely different character, however, in comparative psychology. Here we remain at the inner, the somatic, side of the plane of Fechner. What happens in this material world cannot be executed without transmission of energy. Everywhere a dissipation of energy takes place, either of caloric or of electric energy. Therefore, certainly the time will come, let us hope in a not too distant future, that an energetics of comparative psychology will be created. The problem is one of measurement and calculation, and such problems can always be solved, although the laborious character of the work bars the way.

However, when we wish to put the usual psychology of man on an experimental basis, we have to return to the Spinoza-Fechner parallel hypothesis. Then the conscious resp. subconscious processes remain permanently separated on the outer, the reflex actions on the inner, side, both permanently separated. We have to experiment either on one kind of phenomenon or on the other. We are not sure that the two processes, that on the outer, psychical, and that on the inner, somatic, side of the imagined plane, are perfectly synchronous. Nothing in known about this, we can only suppose it to be arbitrary. This supposition does not facilitate the work, if we wish to attack it on both sides.

Some Aspects of the Future of Psychology. Nobody will expect from me, a guide who is not from the country to be explored, any prophecy about true psychology, but I hope two simple remarks may be tolerated: first, one about the introduction of the methods of comparative physiology in the psychology of man; secondly, one about experimental phonetics as a promising chapter of psychology.

The brilliant lectures of I. P. Pavlov have opened a new period in comparative psychology. The doctrine of conditioned reflexes has become the foundation of behaviorism. By reflexes the animal obtains its equilibrium with the environment, an equilibrium which is continually sought, from birth to death. Therefore, when we wish to understand the behavior of any animal, we have to study its reflexes, such as are evoked by the ordinary and the conditioned stimuli. As a plurality of stimuli is generally received at the same time, facilitations and inhibitions are struggling with one another, and the final result depends on the energies flowing together. By energies are not meant the energies of the stimuli, but the energies taking their origin in the nervous tissues and arriving in the centers along the various paths. They mix together, but not in a simple summation, for the one facilitates, the other inhibits. The complication is incredible, even in animals. Only under very simple conditions can the intensity of such nervous energies be measured, e.g., by a vivisectional study of the degree of an electrotonus pushing back the status of excitation to normality.

Because the higher processes of an osmatic animal are guided by olfaction, the conditioned olfactory reflexes should be studied in a box with movable glass walls, as described above, placed inside a silent room. The animal will be in an odorless environment and olfactory conditioned stimuli can be introduced.

In man perhaps analogous experiments can be executed. More promising, however, in man, are the experiments with the visual and the auditory conditioned stimuli, principally in very young children. In that early period all conditioned reflexes will go on to completion. Later on, the efferent part of many reflexes is gradually inhibited, and from that moment we may suppose that thoughts flow without any accompanying motor or secretory reaction. That which in the beginning evoked an imitation reflex now evokes a pure psychic reaction. The gestures of his fellow creature call up in the child analogous sequences, and the little, thinking individual learns to express in the same manner the thoughts rising up in himself. Soon the mystery of music is felt and, moreover, from the third to the seventh year, the acoustical signals make their entrance in wonderful combinations of tones and noises, which are repeated innumerable times and reach the ear of the child as words, each with its own significance. More and more, the power of speech becomes evident. The grammatical sequence of words prints its stamps on the mode of thinking and, if, later on, the words are repeated by the young individual himself, his inner hearing, which accompanies the speaking attempts, also takes the lead of the logical sequence of his thoughts. There is a deep truth in the sentence of Charles V, who is supposed to have said that each time he travelled in his countries and learned a new language, he won a new soul. It is not possible to think in Flemish, in Spanish, in French, and in German in the same manner. Speech is not only a skilled movement, as gestures are, it is more; it is the scheme in which our thoughts flow. In the later years of these third to seventh years of childhood the influence of art is added. It is said to have existed even in prehistoric times. The Rhodesian man had invented its rules and followed them as now our children do. Religion made its entrance into the so-far developed mind and in historical times it took its highest forms.

Gradually the ideas display themselves in a tridimensional form. The history of Chinese art teaches that its development is reached relatively late; and, also, the little differences between the mode of thinking in people with monocular and in those binocular vision demonstrate that the third dimension is not so preponderant in vision as the two others and can be substituted for by the far less accurate information procured by touch.

An inquiry about the influence of the conditioned reflexes in this long and wonderful development should be very useful. But the development of human intellect does not stop when the representation of three dimensions in the optic and tactile space is obtained; the fourth dimension, that of time, will be added at an early time, when the objects are seen and touched in movement, and the abstract situations are represented and memorized in their changes. Finally, a fifth dimension will be added, when the logical sense of concrete and abstract ideas is varied in a sharply-defined, often-repeated, and well-memorized manner. Only man, I believe, is capable of this kind of thinking, and the conditioned reflexes have lost much of their influence in him. Here, I think the comparative method finds its limit.

My second remark regards experimental phonetics. The theory of Wundt supposed that speech originated as a gesture colored in its affect by tone. From this special point of view, no doubt, speech should also be really a skilled movement, and we cannot wonder that the deaf-mutes learn to speak without hearing in their childhood, when the imitation reflex is but little inhibited. Here the other senses must aid thinking. The hearing man, however, beginning with Pithecantropus erectus, has developed his intellect under the influence of speech. Therefore, phonetics, and *more especially the acoustical phonetics,* have the greatest significance for psychology. A broad and deep perspective extends, as well as a rich field of investigation, though phonetics is but one of the points in the surrounding sciences from which an explorer can start. Therefore, I do not pretend to be a propagator of some favorite method, even if the reader should be inclined to forgive me, thinking: *Navita de ventis, de tauris narrat arator.*

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